BEHAVIOR OF RACCOONS TO A TEMPORAL SERIES OF STIMULI

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Four figures

This problem was suggested by, and the experiments were conducted under, the supervision of, Dr. W. S. Hunter. In his thesis on Delayed Reaction, Dr. Hunter found it necessary to criticise all extant arguments and experiments designed to prove the existence of images in animals. One of the most striking of these experiments is that of Cole,[1] in which raccoons were taught to discriminate between two temporal series of stimuli. It was proposed that we repeat this particular experiment and test some of its conclusions and assumptions in the light of additional control tests.

Cole's apparatus was termed a "card displayer" and his diagram is duplicated in figure 1. It consists of three levers, with cards attached, mounted on a common axis inserted in the rear of a 12-inch board which serves as a screen. By the operation of the lever mechanism, these cards can be raised above the board and displayed as stimuli to the animal. The cards used were white, blue and red. In what may be termed the positive group or series, white, blue and red were displayed in succession, and the animals were taught to react to this temporal series during the presentation of the red card by mounting the steps shown in the diagram. Food was the reward of a successful response. In the negative series, the red

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[1] Cole, L. W. Concerning the Intelligence of Raccoons. *Jour. of Comp. Neur. and Psych.*, 1907, vol. 17.

card was exhibited three times in succession; the animal was taught to respond during the third presentation, and the response consisted of an inhibition, i.e., the animal must remain off the steps. This inhibitory response was inculcated without a food reward.

Three animals learned the discrimination, and Cole makes three questionable assumptions as to *facts:*—(1), The discrimination was mediated through vision; (2), it was based upon card distinctions (presumably differences in color and brightness), and (3), all three presentations of each series were effec-
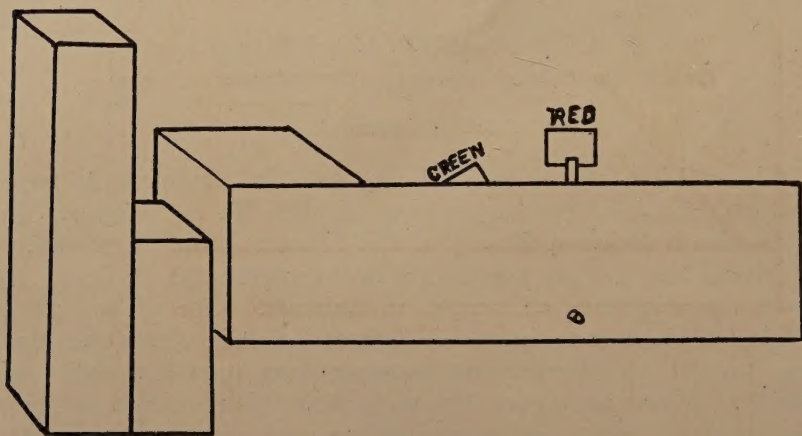

FIGURE 1.   Dr. Cole's color-displaying device and feeding block

tive components of the stimulus. His evidence upon this latter point consists of the fact that the response generally occurred during the presentation of the third card. Since this third member was identical for the two series, it, in and of itself, could furnish no basis for the two differential responses. In case of premature responses, i.e., attempts to mount the steps before the third card was presented, Cole observed that the animal frequently stopped and looked around when this final card was presented. This behavior indicates that the final red card "was not a neglected element of the situation."

The argument in favor of the existence of images takes two forms and each involves a questionable assumption of *theory:*—

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² Op. cit., p. 258.

(1), In normal behavior the responses occur during the presentation of the third card which is common to both series. The responses must then be determined in part by the preceding cards which are no longer present to sense. Hence the *stimulat-*
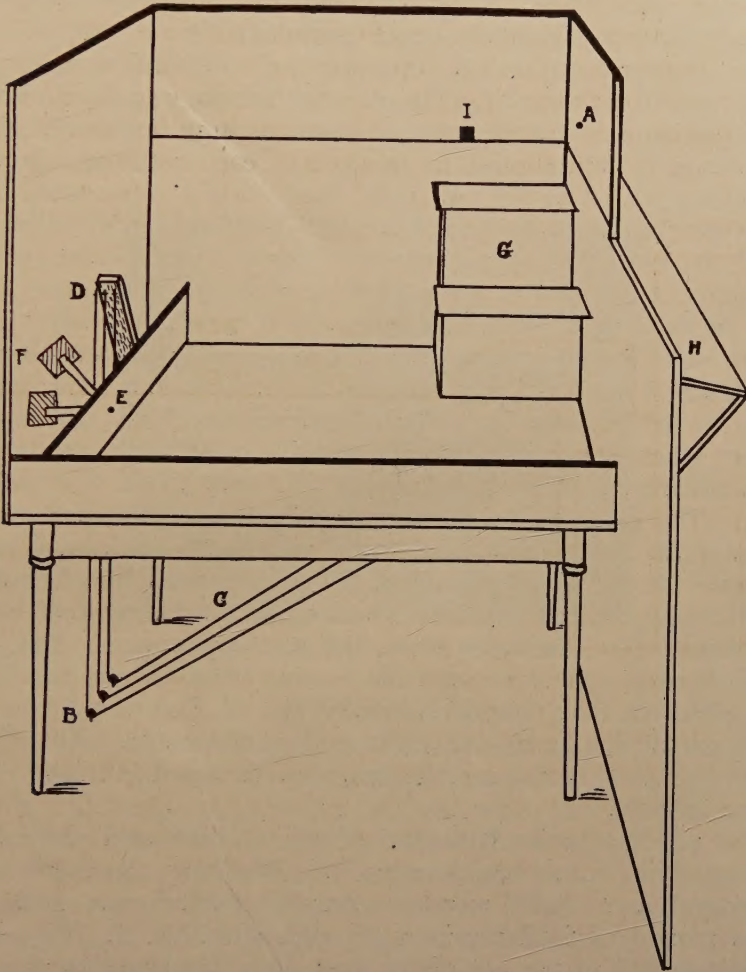

FIGURE 2. Gregg and McPheeters' experiment table and apparatus

ing effects of the first two cards of each series must be *carried over* in some manner until the response does occur. The questionable assumption is now made that this "carrying over" function can be subserved only by an imaginal process. (2),

With the premature responses, the "turning back" proves not only the effectiveness of the final card, but it is assumed that this behavior was *instigated* by an anticipatory image of that card. We are told that this final red was an anticipated and expected color "which they (the animals) generally waited to see but sometimes were too eager to wait for."[3]

The essentials of Cole's apparatus and method were duplicated in our experiment. The main difference consisted of the introduction of a large screen to hide the experimenters. Our apparatus is represented in figure 2. Our operator, usually Mr. Gregg, stood at the rear right hand corner of the table and observed the raccoons through a 1 cm. opening, A. He manipulated the levers, F, by pulling the cords, C, which passed from the ends of the levers down through pulleys on the floor, B, and up to a point within easy reach of his right hand. The levers were limited in their range of movement by elastic cords, D, attached at their lower ends to the table and at their upper ends to an upright bar. This arrangement kept the levers hidden when not in use, and reduced the noise of the operation to a minimum. The board screen, E, was about five inches high. The color cards, F, were two inches square, and when elevated above the board they were practically on a level with the eyes of the raccoons. The distance between the elevated positions of contiguous cards was approximately one-half inch. The levers were similar in form, size and appearance. The distance between the levers and the feeding steps, G, was two feet. The observer and recorder, usually Mr. McPheeters, stood at the shelf at the front and right end of the table. All of his body with the exception of his head was screened from the view of the animals.

The positive series consisted of white, blue and red cards presented in immediate succession. Mounting the steps was considered a successful response and this reaction was rewarded by a morsel of food received through the hole, I. Figure 3 represents Jill in the act of feeding. Any tendency to premature response in this series was punished by the withholding of food. The red card presented three times in succession constituted the negative stimulus. Remaining off the steps was regarded as a successful response, but no food was given either

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[3] Op. cit., p. 258.

for successful or unsuccessful reactions to this stimulus. The experiment offered no means of punishing premature responses in this series.

Two raccoons were used in these experiments, a male, Jack, and a female, Jill. These animals had been previously employed in Hunter's experiments on Delayed Reaction. No factors essential to the solution of the problem were carried over from their previous training. The raccoons were docile, and, after a preliminary period of feeding upon the table, manifested no fear of the apparatus or experimenters.
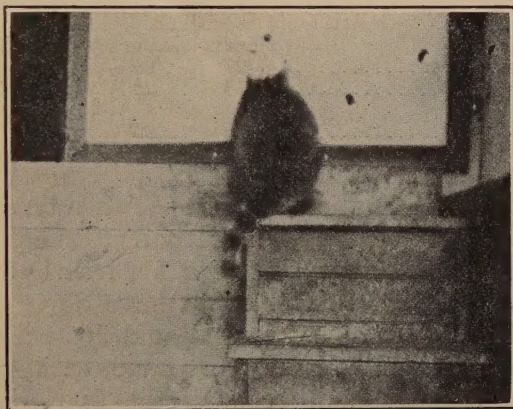

FIGURE 3. Jill in the act of feeding

LEARNING SERIES

The animals were taught to react to the positive series (W-B-R) and to inhibit reactions to the negative series (R-R-R) in the following manner:—(1), They were fed for one week upon the experimental table to accustom them to the novel surround'ngs. (2), They were now taught that food could be obtained only through the screen opening (I) after mounting the steps. (3), In two days they learned to respond whenever the levers (without cards attached) were presented. The animals reacted immediately to the first stimulus resulting from the operation of the lever mechanism. (4), Color cards were attached to the levers and only the positive series (W-B-R) was presented. The animals were forced to delay their response until the complete series was given. Food was withheld for

all premature responses. Fifteen days were. necessary to incul-
cate this habit so that 80% of correctness was secured. (5),
In the final stage the negative series (R-R-R) was introduced
and two weeks was allowed to learn the discrimination between
the two series. At first the positive series was presented the
more frequently. As the learning progressed, the two series
were presented an equal number of times. The two series were
alternated in a definite complicated order. The number of
trials per day varied from 30–50 according to the condition of
the animals. Typical records at the beginning and at the end
of the learning period are:—

	Jack		Jill	
	Trials	Accuracy	Trials	Accuracy
Positive series.....................	27	89%	31	84%
Negative series....................	14	29%	17	24%
Positive series.....................	19	95%	22	91%
Negative series....................	20	0%	22	91%

There was no discrimination at first. Both animals reacted to
the negative series almost as frequently as to the positive series.
Jack's training had simply perfected his reaction to any *com-
pleted* series. Further training *might* have developed discrimi-
native reactions in his case, but time did not permit a contin-
uance of the tests. Jill, on the other hand, easily mastered the
problem and discriminated between the two series on some
basis.

Several peculiarities of behavior were evident during the
learning. (1), During the fourth stage, the animals began to
seek cues from the experimenters. This was particularly true
of Jack. He seemed to watch the peep hole, although possibly
he was merely listening for some sound upon which to base
his reactions. The slightest rustle or movement on the part
of the operator was sufficient to initiate a response. This con-
duct was most evident during the interval between stimuli, or
immediately after the presentation of the third card. In order
to eliminate this extraneous cue, a metronome was kept beat-
ing at a rate of 80 vibrations per minute during the tests. This
noise was sufficient to render inaudible the slight sounds made
by the operator as we had no further difficulty from this source.
(2), The behavior which Cole emphasizes so strongly occurred
very frequently. The animal reacts prematurely, hesitates,

and then turns to look for the third stimulus. (3), During the fourth stage, the animals acquired their fixed modes of behavior. Jack always stood at a position of 6–10 inches from the nearest lever. This position during the presentation of the levers is illustrated in figure 4. Jill, however, soon acquired the habit of standing close to the levers and touching her nose to them as they appeared. (4), When the final stage of discrimination was introduced, Jack's behavior did not change. Very seldom did he inhibit a response. He either reacted prematurely or else reacted at the end of every series whether positive or negative. (5), Very significant changes occurred in Jill's behavior.
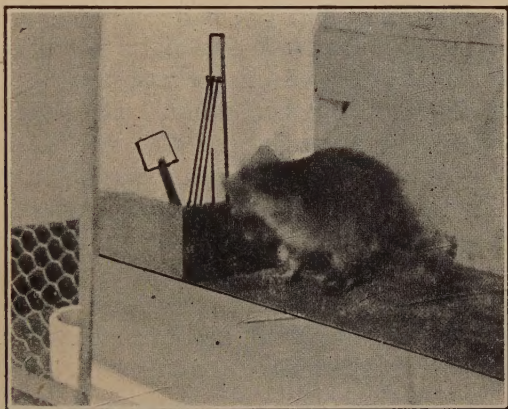

FIGURE 4. Jack in the stimulus position

She soon learned to react immediately after the first presentation of the negative series. Very rarely did she wait for the second card but turned and walked unconcernedly about the table as soon as the first card was presented. Since no food was given in the negative series, the conditions of the experiment offered no means of punishing this prematureness of response. With the positive series, Jill became highly impatient to react immediately after the appearance of the first card, and in fact many premature responses did occur. In this series, however, every premature response was punished by the withholding of food. In all probability immediate reaction to the first card would also have developed in this series if punishment had not prevented. This behavior indicates that the discrimination was

based primarily, if not wholly, upon the initial members of the two series.

CONTROL SERIES

Jack failed to discriminate between the two series, but he did learn to inhibit his responses until the final card had appeared. The control series of tests was instituted to determine the stimulus of these responses. (1), The color cards were removed and the levers alone were presented. No disturbance of normal conduct resulted. (2), The levers were so operated that they made their usual sound while they did not appear above the screen board. The visual-auditory stimulus was thus replaced by an auditory one alone. No change in behavior resulted. (3), A series of but two sounds invariably stimulated the normal response. When a series of four or five sounds were given, Jack always inhibited his reaction until they ceased. Since all possibility of contact stimuli is eliminated by the distant position of the animal from the levers, these controls indicate that *any completed* series of sounds constitutes the sensory stimulus for this raccoon.

To determine the basis of Jill's discriminative reactions, it was necessary to proceed cautiously for fear of destroying or radically altering the coordination. Because of the crucial importance of these tests, they will be described with some detail in the order in which they were given.

1. To test the relative efficiency of color *versus* lever order, the red and white cards were interchanged. The normal order of colors was given and as a consequence the order of lever positions was reversed from that of the normal. Hereafter in describing the order of lever positions, they will be numbered according to their distance from the animal. The results are given in the table.

Stimulus	Positive responses	Inhibitions
W-B-R, 3-2-1	31	9
W-B, 3-2 (premature)	8	
R-R-R-, 1-1-1	17	14

Counting the eight premature responses as correct, we have a percentage of 67 in 79 trials on the assumption that the reactions were made to *color order*. These results are very poor as compared with previous records. On the preceding day the percentage of correct behavior was 91, and the average per-

centage for the six preceding days was 87. Evidently there is some connection between "lever order" and the ability to react correctly.

2. The normal order of colors and levers gave an accuracy of 87% for 15 trials, which is a normal record.

3. Mr. Hunter operated in the place of Mr. Gregg, and secured an accuracy of 86% for 7 trials with the positive series and a percentage of 89% for 9 trials with the negative series.

4. Mr. Gregg operated, the levers were washed, fresh color cards used, and the normal order of presentation was given. Nine trials gave an accuracy of 89%.

5. The red and white cards were again interchanged in position, and the two series consisted of R-B-W, 1-2-3, and R-R-R-, 1-1-1. The animal was fed whenever it chose to react to either series. Both series stimulated positive responses. Discrimination was absent. There were but two inhibitions in 16 trials and both of these were in response to the R-R-R series.

6. The normal color and lever order was given and a discriminative accuracy of 93% was secured in 15 trials.

7. Mr. McPheeters operated in the place of Mr. Gregg, as the latter still retained a suspicion that the animal might be reacting to some difference in his method of giving the two series. The normal color and lever order was adhered to. An accuracy of 90% in 10 trials was secured.

8. The normal lever order of 1-2-3, and 3-3-3 was given, but the positions of the cards were so shifted that the colors were presented in the order cf R-W-B, and B-B-B. Jill was fed only after response to the series R-W-B. Assuming that the discrimination was based upon differences of "lever position," this test gave an accuracy of discrimination of 90% in 20 trials. Evidently the shift of presentation order of the cards did not disturb the discriminative reactions in the least.

9. Violet, green and yellow cards were substituted for the white, blue and red cards respectively. The normal lever order was adhered to. This change rendered both series different from the normal in color and brightness. No disturbance in behavior resulted. The two series were discriminated successfully in 86% of 21 trials.

10. The cards were now removed and the bare levers were presented in their normal order. The animal successfully dis-

criminated between these two groups of lever positions in 88% of 17 trials.

11. The three levers were practically identical so far as form, size and appearance are concerned. To test the possibility that the levers were distinguished on the basis of their individuality as tactual or visual objects, rather than on the basis of their positional differences, the levers were interchanged in position. Lever 1 was placed in the second position, lever 2 in the third position, and lever 3 was given the first position. The cards were removed, and the normal order of position was given. No disturbance resulted.

12. The original series, both of colored cards and of lever order was alternated throughout two days' experimentation with a series in which the cards were removed but the normal lever order maintained. These tests were introduced to substantiate more thoroughly the contention as to the ineffectiveness of the cards. The tests prove that the presence of the cards is not only unnecessary for discrimination but also that their presence does not materially increase the accuracy of that discrimination. An accuracy of 94% was secured for 55 trials when the cards were present. Absence of the cards gave a percentage of 92 for 53 trials.

13. This test was designed to determine whether the "positional" difference between the two temporal series of lever presentations was apprehended by means of vision or touch. The color cards were used and the normal lever order was maintained. A plate of window glass 18 x 24 inches was placed immediately in front of the levers. This glass effectually prevented all contact, but it in no way disturbed vision. The animal was allowed 20 minutes to become accustomed to the new situation. Neither timidity, nervousness, nor curiosity were manifested. During the two days' experimentation, tests were taken both with and without the presence of this glass, all other conditions remaining constant. Without the glass, behavior remained normal in every respect. The presence of the glass, however, destroyed the coordination. The animal's attitude was one of bewilderment. The first 11 trials were all incorrect. The raccoon soon learned to react either to the visual appearance or noise of the levers, but she responded to every presentation, both positive and negative. In other words inhibition to the negative series ceased; discriminative ability was destroyed.

The above series of tests prove rather conclusively that (1), the animal was not dependent upon the operator's method of manipulating the lever mechanism; (2), neither the brightness, color nor presence of the cards are necessary to the discrimination; (3), the animal is not reacting to individual peculiarities of the levers; (4), the discrimination is based primarily upon an apprehension of a difference of spatial position between the two groups of levers; (5) this apprehension of the spatial difference between the two groups is not mediated through the olfactory, auditory, or visual modalities of sense; (6), this difference between the two lever groups was perceived by means of nose contact, for, (a) all other sensory possibilities were eliminated, (b) nose contact was present throughout the tests and (c) discrimination failed completely when such contact was prevented; (7), the discrimination was based upon active rather than passive touch. The animal did not hold the head in a constant position and allow the ascending levers to strike at a series of positions on the cheek. The head was moved so as to receive each contact upon the nose. As a consequence, we are forced to infer that the kinaesthetic sensitivity involved in reaching for the stimulus was probably more effective than pure contact in these discriminative responses.

14. The positional difference between the levers upon which the discrimination was based was one-half inch. The thickness of the levers was one-quarter inch. Levers made of thin sheet iron were now substituted. By the use of washers the above positional differences were maintained. A series of 117 trials gave an accuracy of 90%. Most of the errors occurred in the negative series and during the early trials. The change of material and thickness of the levers thus produced, as might be expected, a slight temporary disturbance which was soon eliminated.

15. The positional difference was now reduced to a minimum by removing the washers between the thin sheet iron levers. The purport of the control is obvious. The experiment, however, was not satisfactory as the levers would catch and bind, and time was consumed in disengaging them. The animal soon became confused, excited and impatient to react. The significance of the results is thus ambiguous. Irrespective of their interpretation, the factual results are:—the percentage of pre-

mature responses was significantly increased, the positive series was reacted to correctly, while the negative series gave more positive than negative responses. Such results might naturally be expected from the animal's excitement and impatience. In the few cases where the levers worked smoothly and the animal seemed in a normal mood, discrimination failed for the minimum of spatial difference. However, we must confess that our test was not decisive.

16. A final series of tests covering three days of experimentation was given (1) to determine the composition of the two lever groups which renders them effective as specific stimuli, and (2) to determine the relative effectiveness of the three members constituting any lever group. Since the cards are ineffective, they were not removed. Dr. Hunter operated in the enforced absence of Mr. Gregg. The results are given in the following tabular statement.

Stimulus	Positive	Inhibitions	Percentage of discrimination
W-B-R (1-2-3)........................	44	2	
R-R-R (3-3-3)........................	5	27	91.
W-W-W(1-1-1)........................	13	0	
R-R-R (3-3-3)........................	1	13	96.
W-R-R (1-3-3)........................	11	0	
R-R-R (3-3-3)........................	1	9	95.
B-B-B (2-2-2)........................	6	0	
R-R-R (3-3-3)........................	1	3	90.
W-W-W(1-1-1)........................	25	0	
B-B-B (2-2-2)........................	6	14	87.
W-B-B (1-2-2)........................	23	5	
B-B-B (2-2-2)........................	4	13	81.

As to the character of a group constituting a specific stimulus we may say:—(a), The group is highly general, never definite and particular. Positive reactions of food getting may be stimulated successfully by any of the following groups— 1-2-3, 1-3-3, 1-2-2, 2-2-2, or 1-1-1. Likewise, inhibition, or negative responses, may be stimulated by either group 3-3-3, or 2-2-2. (b), The nature of either stimulus is relative to the character of the other group with which it is alternated. The group 2-2-2 may serve equally well either as a positive or negative stimulus. It excited the negative response with an average

correctness of 73% in two of the above series; it likewise func-
tioned on one occasion in inducing positive responses with a
correctness of 100%. (c), When two groups are alternated,
that nearest to the animal serves as a positive stimulus, while
the more distant one becomes the negative stimulus. The
group 2-2-2 was positive when alternated with 3-3-3, but it
functioned negatively when given in conjunction with 1-1-1.
(d), The effectiveness of any two groups in inducing differential
responses is proportionate to their distance apart. The groups
1-1-1 and 3-3-3 were discriminated with an accuracy of 96%
for 27 trials, while the above two groups were distinguished
from 2-2-2 with an accuracy of but 87% for 55 trials.

The above principles may be stated in terms of positions as
follows:—When two groups of positional stimuli are alternated,
the one nearest to the animal becomes positive while the more
distant one is negative. The accuracy with which the two
groups may be discriminated is proportional to the degree of
depth distinction between them. The animal thus responds to
objects according to their relative position in depth.

As to the relative effectiveness of the three members com-
prising any group stimulus, our results indicate with a high
degree of probability the following conclusions:—(1), The dis-
crimination *can* be based *exclusively* upon the difference between
the first members of the two groups. Only these initial factors
were effective in a majority of the tests. This conclusion is
founded upon several groups of facts. In no. 16 of the control
tests, 66 trials were given in which the last two members were
identical in the two discriminated groups, yet these trials gave a
discriminative accuracy of 85%. Moreover, this accuracy was
proportionate to the degree of space difference between the
two first members; for example, the groups 1-3-3 and 3-3-3
gave an accuracy of 95% as compared with a percentage of
80 for such groups as 1-2-2 and 2-2-2. When two groups are
identical as to the first member but different in respect to the
last two positions, discrimination is impossible. In no. 5, the
groups 1-2-3 and 1-1-1 were distinguished in but 12% of the
trials, a record which can be accounted for on the basis of
chance. As before noted, the discrimination was mediated
through contact and the first member of each group was usually
nosed. The inhibitory response to the negative series was

finally made *immediately* after the first presentation. With the positive series, the animal became impatient to react after the first presentation and in fact many such premature responses did occur. In all probability immediate reaction to the first member would also have developed if this tendency had not been punished by the withholding of food. (2), The final member was never effective in the discrimination. The conclusion is supported by the facts already cited as to the tendency to correct premature responses to the positive group, and the usual reaction after the first presentation of the negative group. This third element was common to the two groups in the original experiment, and logically it is obvious that a common element can not serve as a basis of distinction. This training on the original series would naturally tend to make the animal neglect this final member in those cases where it did offer a possible basis for discrimination. · As a matter of fact those groups which did differ as to the third component gave a discriminative accuracy no higher than the normal. (3), While the third member is useless so far as discrimination is concerned, yet it did possess a function in the positive group. As Cole asserts, this component "was not a neglected element of the situation." [4] It functioned, however, merely to *release* the discriminative response which had already been aroused by the preceding members of the group. (4), As to the efficacy of the second member, our results are far from conclusive. Exigencies of time at the close of the college year prevented an adequate completion of the tests. The facts as to premature responses indicate that this element was sometimes effective in the positive series, but that it constituted no part of the negative stimulus. In test no. 5, the groups 1-1-1 and 1-2-3 were both regarded as positive stimuli, but the percentage of responses was slightly greater in the latter case. This fact indicates a slight effectiveness of the second element. Some results of test no. 6 corroborate the assumption. A·comparison of the last two groups shows that the accuracy of discrimination is slightly greater when the two series are different as to the second member than when they are identical in this respect.

The conclusions thus far advanced are consonant with all of the facts except those of the first control test. With the

[4] Op. cit., p. 258.

groups 3-2-1 and 1-1-1, we would expect a high degree of dis-
crimination, with the group 1-1-1 being regarded as the positive
stimulus. As a matter of fact the animal was confused and
unable to give consistent results. Both stimuli were sometimes
regarded as positive and sometimes as negative, though the
tendency to a positive selection was the stronger in both cases.
The group 3-2-1, however, was selected as positive much more
frequently than was the group 1-1-1. These results offer three
possibilities of interpretation:—(a), An animal may adapt itself
to the alterations of the discriminative stimuli which are intro-
duced in the control tests. In this way the nature of the effec-
tive stimulus may be gradually altered. By interpolating but
a few controls in a longer series of the normal, one can prevent
any radical alteration except as to some minor details. For
example, much of the relativity of the two group stimuli in
our experiment was probably acquired during the controls. The
anomalous results of the first test can be explained upon the
hypothesis that the discrimination was originally based upon
both color and lever order, the breakdown was due to the func-
tional opposition of the two factors, and the element of color
order was subsequently neglected. There are several objections
to such a hypothesis. The visual element must have been
stronger than the factor of position to account for the strong
positive tendency in the responses, and it is improbable that
the stronger component should be subsequently neglected. The
change or adaptation should be gradual and there is no evi-
dence of this. The animal which stood in such a position that
lever contact was impossible failed to learn the discrimination
on a visual basis. (b), The first alterations of any accompany-
ing conditions upon which the discriminative responses do not
directly depend may cause confusion and disturbance and the
animal may soon learn to adapt itself to these novel disturbing
conditions. There was much in the behavior of the raccoon
which suggested such a hypothesis. (c), The results may also
be explained by additional assumptions as to the nature of
the group stimuli, which in no way conflict with our previous
conclusions. We may assume that both the first and second
members of the group are sometimes effective, that the animal
tends to react positively to any spatially distinct series of stim-
uli, and negatively to any series of identical members. While,

as we have asserted, the two groups are relative to each other, yet they cannot be completely relative. The stimuli must also possess certain fixed, definite and absolute attributes and this feature is furnished by the above characteristics of difference or identity of component elements. The group 3-2-1, given in conjunction with 1-1-1, would be selected as negative so far as the animal was depending upon the factor of relativity of depth distinctions, but it would likewise be regarded as positive in so far as the animal was influenced by this definite characteristic, viz., a series composed of different elements. The anomalous results of the first test are thus explicable on the assumption that the two effective factors of each group stimulus were brought into antagonism. In the normal series, one stimulus invariably consisted of a succession of identical elements (3-3-3), while the other stimulus was invariably composed of a succession of different elements (1-2-3). One has a right to assume that the animal in learning to distinguish the two will rely to some extent upon every available distinctive aspect —upon the factor of identity *versus* difference of elements as well as upon relative nearness or distance of the groups. The writers incline to this hypothesis because it harmonizes with and supplements the conclusions previously enunciated. This explanation of the results of test no. 1 furnishes additional evidence in support of our previous contention as to effectiveness of the second member of the group stimulus.

It is evident from our results that Cole's conclusion as to a discrimination based upon visual peculiarities of the cards is an unwarranted assumption. He did not (so far as can be determined from his account) eliminate the possibility of a discrimination based upon cues from the experimenter, or upon his method of lever manipulation. The discrimination may have been tactual as in the case of our animal. Cole describes his animals as standing with forepaws upon the card displayer and clawing at the levers, and our raccoon which stood in such a position that contact was impossible failed to master the problem. However, we do not wish to make any conclusions as to Cole's animals; we merely wish to point out the naïve anthropomorphism underlying his statement of fact.

His conclusion as to the efficacy of the third lever in the positive series is valid, but he fails to distinguish between a

discriminative and a "releasing" function. We are sceptical as to the efficacy of the final levers in his negative series; in speaking of the negative series, he says "for each one, on seeing the first red, would drop down from a position with both forepaws on the front board to stand on all fours on the floor in front of it and merely glance up at the succeeding reds." [5] Remember that the experiment offered no means of punishing premature responses in this series, and that our animals finally reacted immediately after the first presentation. It is rather difficult to identify a negative or inhibitory response and we see no reason why this climbing down from the card displayer should not be regarded as the negative response.

Our factual criticisms, however, do not invalidate Cole's argument in favor of imagery. His logic applied to our facts would merely substitute kinaesthetic-tactual for visual images. As formerly noted, his two arguments involve theoretical assumptions. (1), His animals as well as ours during the course of a premature response frequently stopped and went back to the card displayer when the third lever was presented. Cole assumes that this behavior was initiated by an image. The assumption has two weaknesses. (a), It is more probable that the act was stimulated by the noise of the lever. The raccoon is very sensitive to sounds and any noise would naturally possess a distractive function. This sound has been an invariable component of the stimulus associated with food satisfaction, and hence it would soon acquire motive power. Punishment of premature responses would strengthen the effect of such a stimulus. The assumption of images to explain such conduct is entirely gratuitous. (b), Granted the existence of images, one may argue with some plausibility that they would function to *prevent*, and not to arouse, the conduct in question. On the assumption that the original function of an image is a substitute for the corresponding sensation in a sensory-motor situation, one would not expect the animal to turn back to secure a sensory stimulus for which it already possesses an adequate substitute. (2), As to the second argument, Cole is correct in maintaining that the stimulative effects of the levers upon which the discrimination was based must have persisted.

[5] Op. cit., p. 260.

in some fashion until the final presentation was given. This "carrying over" function was probably mediated, not by an image, but by motor attitudes representative of, or associated with the two responses. One can not make positive assertions as to the character of these motor attitudes. One can point out peculiarities of behavior which could well serve such a function. Our animal during the positive series maintained an attitude of tension and excitement, while a degree of relaxation was evident in the negative series. Cole's animals during the positive series remained with forepaws on the card displayer, pawed at the levers and exhibited a state of tension and excitement; with the first presentation of the negative series, they dropped down from the card displayer, maintained an attitude of relaxation and indifference, and casually glanced at the succeeding levers. It is perfectly feasible that two such distinctive motor attitudes might serve as the stimulus or beginning of subsequent appropriate modes of response. If images were present in our animal they must have been kinaesthetic, i.e., imaginal attitudes. Conceived in this manner, the probability in favor of *sensory* attitudes is more convincing. We have described the function of the third lever as one of "release," i.e., we assumed that the first levers determined or initiated the appropriate response, that the conditions of the experiment prevented the immediate completion of the act, and that the final presentation merely released this inhibited movement or motor attitude. This conception is supported by the fact that the animals exhibited a pronounced tendency to immediate or premature responses, a tendency so strong and impelling that punishment never succeeded in wholly eradicating it.

The primary concern of this paper, however, is not to establish any positive doctrines. We expressly refrain from asserting that raccoons can not and do not possess and utilize images. Our purpose is essentially negative and critical; we desire to demonstrate the inadequacy of Cole's experiment as a conclusive and convincing proof of the existence of images. The assumption of higher processes is both unconvincing and futile so long as there exists even the possibility, to say nothing of the probability of an interpretation in terms of lower and more primitive conditions. The existence of images must remain

unproved so long as an experiment, by the absence of appropriate control tests, fails to eliminate the possibility of a "stimulus-response" type of behavior, and we maintain that this possibility of a sensory-motor interpretation of the raccoon's behavior in this discrimination has been rather adequately demonstrated by our tests.

THE OLFACTORY REACTIONS OF THE SPOTTED NEWT, *DIEMYCTYLUS VIRIDESCENS* (RAFINESQUE)

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Although olfactory reactions have been recognized in fishes by Baglioni, Parker, Sheldon and Copeland, to my knowledge, no conclusive physiological evidence of a sense of smell in· amphibians has yet appeared. In the May-June (1912) number of this Journal, Reese published an article on food and chemical reactions of the spotted newt (*Diemyctylus viridescens*), but failed to show reactions unquestionably dependent on olfactory stimulation. I began a study of the sense of smell in this species two years ago, but was unable to complete it until last summer, while occupying a table at the Laboratory of the United States Bureau of Fisheries at Woods Hole, Massachusetts. I wish to express my thanks to Mr. T. E. B. Pope, Director of the laboratory, for many courtesies received during my stay, and to Mr. P. H. Pope of Manchester, Maine, for his kindness in supplying me with abundant material.

The spotted newt is especially favorable for experimental study as it soon becomes accustomed to life in the aquarium, feeding well, and showing no ill effects from confinement. All my animals were kept in glass aquaria, with sand spread over the bottoms, and were fed from time to time with pieces of raw beef, which were offered to them on the end of a delicate silver probe.

EXPERIMENTS

As an introduction to the feeding behavior of Diemyctylus, one of my earlier experiments may be described; a series of tests originally planned to throw light on the animal's ability to recognize food. The individual chosen for the tests indicated that it was hungry by seizing and swallowing a piece of raw meat offered it on the tip of a probe. When a ball of white cotton was substituted for the meat, it also was taken into the mouth, but immediately dropped. Another one was seized,

chewed for a while, and discarded. Instead of testing the animal again with meat, an extract was prepared by grinding raw beef in water and filtering the fluid. A piece of cotton was then soaked in this juice, and offered the animal as before. By this procedure the introduction of new visual or tactile factors was practically avoided, for the color of the cotton was changed only slightly by the juice. It was seized, chewed upon and swallowed. Plain cotton was then refused. When the cotton was moved the newt followed it about, resting the tip of the snout upon it. Cotton soaked in juice was then nosed in a similar way until it dropped off the probe. When rolled over the sand, the cotton ball was pursued and again nosed. Finally it was snapped into the mouth and swallowed. Plain cotton, presented to the animal in exactly the same way, was nosed but not taken. Lastly, another piece soaked in juice was offered. As in the two preceding tests, the animal nosed it several times but did not seize it. When moved with the probe, it was taken and swallowed. A piece of meat was then refused.

A study of the behavior exhibited in this experiment reveals a number of reactions which were found to be characteristic of other individuals under appropriate stimulation. First, there is an approaching or following reaction. The hungry animal approaches a motionless object, or follows one moved in front of it. Secondly, there occurs a nosing reaction, which consists of placing the anterior end of the snout, where the external nares are situated, on the object under investigation, where it may be held for several seconds. This reaction is strongly suggestive of "smelling," and is one of the most striking occurrences in the feeding behavior of the newt. Thirdly, the object may be seized or snapped at, and lastly, if taken into the mouth, it may be swallowed. The problem lay in determining whether any one of these reactions is initiated by a stimulation of the olfactory receptors.

The approaching reaction. The approaching reaction was first studied. The importance of sight in this response was easily demonstrated. If a small piece of meat on the end of a wire was moved about in the water four or five centimeters in front of an animal, it was eagerly pursued. When one approached the meat, others usually followed, until all the occu-

pants of the aquarium nearby became alert or active. If, next, a roll of filter paper was substituted for the meat, it, in turn, was followed in the same way, and the response to the inedible object was just as marked when the meat was not introduced at all; in fact, paper moved along the outer surface of the glass aquarium was watched and followed.

Finally, an experiment was devised to test reactions to a stationary object, and to show whether meat would be found more quickly than something from which no material capable of producing chemical stimulation could possibly emanate. A piece of raw beef was hooked on to the end of a wire fastened into a wooden bar, which was wedged between the sides of the aquarium above the surface of the water. In this way the meat was held in a fixed position in the water a few millimeters above the bottom of the aquarium, in which were placed five newts. The time elapsing between the introduction of the meat and its discovery by one of the animals was recorded. Care was taken that the animals should not see the meat as it was being placed in position, and thus be attracted by its movement. When the bait was discovered, nosed and snapped at, or seized, it was removed before it could be eaten, and in no two consecutive trials was it placed in the same position in the aquarium. In five trials, the average time taken to discover the meat was 1.8 minutes. Five trials were then made with the substitution of a ball of filter paper for the meat, when the average time taken for its discovery was 3.3 minutes. Upon repeating the experiment, the results were reversed. The average time taken to find the meat was 2.4 minutes, whereas the filter paper was located in 1.2 minutes. In the ten trials, the difference in the average times taken to find meat and filter paper was not more than a few seconds.

All these tests, and subsequent ones, indicate that the approach to an object, edible or inedible, is a visual reaction, and that, under the conditions described, if smell plays a part in food recognition, it does so after the animal has discovered and moved to the source of the stimulus.

The seizing reaction. In the reactions to the stationary meat and filter paper one significant difference was noted. Whereas the meat was always nosed and seized, or snapped at, the filter paper, although invariably nosed, was bitten only five times in

the ten trials. In the first experiment with cotton, plain and soaked in meat juice, when there was practically no difference in the appearance of the two pieces offered, similar results were obtained. In both experiments, however, discrimination be-tween the inedible and edible objects (if cotton soaked in beef juice may be termed edible) was not perfect. The plain cotton was taken twice, and the filter paper snapped at five times. Other things, therefore, besides chemical stimulation may call forth the seizing reaction.

When the object, with which the test is being made, is fast-ened to the end of a probe, and is offered in that way to the animal, the movement of it becomes a possible factor in deter-mining the response. Accordingly, the effect of motion of the proffered object was next investigated. A ball of filter paper, hooked to the end of a wire, was held firmly in position beneath the water by the method already described. A newt was then allowed to wander about the aquarium until it approached, nosed and deserted the filter paper. The paper was then either fastened to a probe and moved about near the animal, or it was rolled over the sand on the bottom of the aquarium. Several tests were made, involving three individuals. In three cases the movement of the filter paper induced the seizing reaction. One animal snapped it from the end of the probe four times in succession, and the other two not only seized but swallowed it. An inedible object in motion, therefore, may be taken into the mouth or even swallowed, whereas the same thing stationary is nosed and rejected. An explanation of this behavior is offered on a succeeding page.

Finally, as noted above, a fixed piece of filter paper is some-times seized. Several factors may be influential in exciting this reaction, but the most important one, I believe, is hunger. An animal, which undoubtedly is very hungry, may cease tempo-rarily to use its powers of discrimination, and seize and swallow filter paper almost as readily as meat, even when the former is not in motion. When, however, such an animal is fed, a different reaction may be obtained. The filter paper ball is then approached and nosed, but not seized. The following tests serve to illustrate the effect of both hunger and a moving object on the newt's feeding behavior. An animal took several pieces of filter paper from a wire in a fixed position. One or

two of these were swallowed. It was then fed with meat, after which it nosed, but refused to snap at stationary filter paper. The paper was next moved about in the water, when it was actively followed and seized.

From the experiments described above, it may be seen that, in order to test satisfactorily the relation between chemical stimulation and the seizing reaction, several factors must be eliminated. The desired results were obtained in the following way. Five animals, isolated in an aquarium, were carefully fed, so as to prevent any danger of excessive hunger. Two wires were fastened to a wooden bar so that their free ends reached nearly to the bottom of the aquarium when the bar was laid across the top. Two cheese cloth bags, 9 to 12 millimeters in diameter, were next made, one of which was filled with meat, and the other with cheese cloth. The bags were then tied to the ends of the wires, and the cross bar placed in position. By this method, two bags of approximately the same size and appearance, were held about 33 centimeters apart, a few millimeters above the bottom of the aquarium. The reactions of the newts to the bags were then watched for a half hour, the positions of the two being exchanged at the end of fifteen minutes. The result of this experiment was as follows. The bag containing meat was approached and nosed about 29 times, and bitten as many as 85 times. Since, frequently, three or four newts were snapping at the bag at once, an accurate record was difficult to obtain. Reactions to the cheese cloth bag were quite different. It was approached and nosed 17 times, but no attempt was made to seize it. As the bag was being lowered into position, it was snapped at by one of the animals, a response initiated by its movement. Three hours later, the experiment was repeated with similar results. The baited bag was approached and nosed 22 times, and bitten 94 times, whereas the other one was nosed 9 times and not once seized. In neither experiment was a case observed where an animal carefully nosed the baited bag and then failed to snap at it. These tests show conclusively that Diemyctylus is able to discriminate perfectly between two bags, one containing meat and the other not, and that the food sensing occurs after the bag is approached and before it is snapped at, or taken into the mouth.

The question next arose: Were the olfactory organs involved

in the reactions just described? To answer this question it became necessary, first, to ascertain in what way the stimulus reaches the receptors, and, secondly, after preventing any possibility of olfactory stimulation, to compare reactions then with those already recorded.

Certain intermittent mouth movements of Diemyctylus are conspicuous. These consist of a rather slow expansion of the floor of the mouth, followed by a sudden contraction, at which time the mouth is slightly opened. If carmine suspended in water is squirted from a pipette over the snout, it is drawn in through the external nares as this expansion progresses, and expelled from them, and from the mouth, when the contraction follows. Normally, therefore, any solution capable of producing olfactory stimulation flows through the nasal chambers, passes through the internal nares, and enters the mouth, and later a portion of it is expelled in a reverse direction by the same paths.

To prevent the flow of water through the olfactory chambers, the snout was first thoroughly dried, and then the external nares were covered with a thin layer of a celloidin preparation know commercially as "Cur-a-cut." The day after experimenting with the bags, the five animals tested were treated in this way. On the following morning one of them had lost the cap of celloidin, through shedding its skin, and another took no interest in meat which was offered it. The remaining three followed and snapped at meat, and exhibited normal behavior in every way. These three were tested for an hour with the two bags as before, the positions of the two being exchanged every fifteen minutes. The reactions to the bag containing meat were as follows. Animal A nosed it 3 times but did not bite it. B nosed it twice and left it both times. C nosed it 8 times and deserted it in every instance except the first, when it was snapped at about 10 times. The cheese cloth bag was nosed once by A, and twice by C. Five hours later the experiment was repeated, after all three had followed filter paper attached to the end of a probe. A nosed the cheese cloth bag once, B nosed it once and the baited bag 3 times, and C nosed each 4 times. Once C, after nosing the baited bag, snapped at it 8 or 9 times, but in all other cases there was no seizing reaction. Thus, in the 20 responses to the bag containing meat, the seizing reactions oc-

curred only twice, and an explanation of the exceptional be-
havior of C may probably be found in the following observation.
Before the bag was bitten in the second experiment, the animal
nosed it, and then, directing its snout downward, expelled
water from the mouth; a fact noticed by the movement of the
sand on the bottom of the aquarium. It then turned to the
bag and attempted to seize it. Tests with carmine showed that
an animal with its nostrils covered draws in and expels a small
amount of water through the mouth. In view of these and
subsequent observations, it seems quite likely, therefore, that
in the two instances recorded material emanating from the
meat was taken into the mouth in considerable quantity, and
reaching the nasal chambers by way of the internal nares,
stimulated the olfactory receptors.

The celloidin caps were removed after the experiment last
described, and on the following morning the three newts were
tested for an hour as previously. Both bags were nosed several
times by B but neither was bitten. A, on one occasion, snapped
at the baited bag after nosing it, and another time failed to do
so. The cheese cloth bag was nosed but not seized. C nosed
the baited bag carefully 4 times, and snapped at it in each
instance. Once a hasty nosing of the bag was not followed by
the biting reaction. This experiment was rendered somewhat
unsatisfactory on account of A and C shedding their skins, an
operation which always interferes with feeding. Five hours
later, when the skins had been cast and the nasal organs given
a longer time for recovery from the effects of non-use, the
animals were again tested for an hour. The reactions of all
were much the same so they need not be described individually.
The cheese cloth bag which was nosed 21 times was not bitten
once, whereas the baited bag, nosed 17 times, was snapped at
56 times. In but a single instance was the nosing of the bag
containing meat unsucceeded by the seizing reaction (when the
response was a nosing of the sand beneath the bag); in short,
the newts had regained their normal ability to sense concealed
food.

Results in accord with those set forth above were obtained
by another method. As Reese showed, Diemyctylus usually
responds to an extract of raw beef by snapping at it directly
or at the sand upon which it settles. By means of a pipette

some meat juice, prepared by the method already mentioned, was squirted over the nostrils of a newt in such a way that the animal could not see the pipette. This was done ten times, and each time the response was a snapping of the jaws. Ten trials with water alone caused no reaction. At the conclusion of these tests the external nares were covered with "Cur-a-cut." Three quarters of an hour later the animal snapped at meat offered it on a probe, followed it about the aquarium, and in no way gave evidence that the presence of the celloidin cap was disturbing. It was then tested ten times with meat juice which called forth no response. The tests were repeated over an hour later with similar results: the snapping reaction failed to occur, although the animal followed and bit at filter paper in a normal way. The "Cur-a-cut" was next removed, and after 15 minutes had elapsed, 10 tests were made with meat juice. A positive response occurred four times. Twice the juice was snapped at, once the sand, and once the sand was nosed. An hour and a half later the reactions to the extract were the same as in the first tests of the experiment, viz., 10 trials resulted in 10 snapping responses. Another individual gave a series of reactions differing only in detail from the last. Four hours and a half elapsed after the nostrils were covered before the animal responded normally to filter paper and meat. Twenty tests with meat juice then failed to induce the usual snapping reaction. Five hours after the "Cur-a-cut" was removed, it was tested with the juice, when it responded six or seven times in 10 trials. A third animal reacted to the meat extract somewhat erratically on the first and second day after the nostrils were covered. Upon examination it was found that the "Cur-a-cut" had loosened so that one nostril was practically uncovered.

From the results obtained in these two types of experiments, one may fairly conclude that the usual snapping at beef extract, or at a bag containing meat, is a reaction initiated by a stimulation of the olfactory receptors, and that when an object is nosed, it, in truth, is being tested by the sense of smell. There is, however, another possible interpretation of this behavior, although one not at all probable. It is conceivable that the stimulating materials may pass through the nasal chambers and internal nares and affect the gustatory receptors of the mouth, and that the failure of the animal to respond

when the external nares are covered is really due to the lack of stimulation of the oral receptors, or perhaps to an insufficient stimulation of them, for, as already stated, a certain amount of water unquestionably is taken into the mouth when the nares are sealed.

In order to determine conclusively which interpretation of results was the correct one it became necessary to sever the olfactory nerves, an operation accomplished with little difficulty. A small opening was made in the roof of the cranium above each nerve, a fine pointed scalpel was inserted, and the cuts made. Notes made on the behavior of a newt with its peripheral olfactory apparatus rendered inoperative in this manner are as follows. Before the operation, which was performed at 10 A. M. on July 24, it snapped at meat, filter paper and meat juice. 10:13 A. M.—The animal showed no ill effects from the operation whatsoever. It followed and snapped at meat, and pursued a ball of filter paper rolled over the sand, a characteristic response of the normal individual. Meat juice, however, induced no reaction in five trials. A piece of meat was then immediately seized and swallowed. 10:30 A. M.—Meat was pursued and snapped at as before, but in five trials with meat juice no reaction appeared. It then followed meat moved through the sand, when the juice again brought forth no response in five trials. 11:15 A. M.—By squirting a mixture of carmine and water over the olfactory apertures it was clearly demonstrated that the operation had in no way interfered with the normal flow of water through the nasal chambers and the mouth. July 25, 8:30 A. M.—The newt followed a wad of filter paper in characteristic manner. 9:50 A. M.—Several tests were made with meat juice without response. A piece of meat was actively followed before, between and after the tests with beef juice. 3:50 P. M.—A bag filled with cheese cloth was nosed as normally. Then one containing meat was substituted for the first, and the animal induced to approach by moving it. It was nosed for many seconds and then deserted. (Another animal with functioning olfactory organs nosed the same bag and snapped at it actively.) A roll of filter paper was immediately pursued and seized, when it was taken away and the animal fed with meat.

The behavior of two other individuals whose olfactory nerves

were cut need not be reported in detail. The day after the opera-
tions both took meat from a probe and followed and snapped at
filter paper. If there were any general ill effects from the opera-
tions they could not be detected through any modification of
behavior. Tests with carmine indicated that the flow of water
through the nasal organs was normal. Both failed absolutely
to respond to meat juice squirted over the olfactory apertures,
although before the nerves were cut their reactions to it were
very pronounced. The effect of a moving object on the be-
havior of an animal deprived of the sense of smell was most
striking. In one instance it was only with great difficulty that
the seizing reaction could be induced, for the newt persisted in
its attempts to test the edibility of the meat by smell. It was
nosed constantly on being moved through the sand, but not
taken into the mouth. Finally, after continued agitation, it
was seized and swallowed.

The results of the experiments last described indicate that
the olfactory receptors, and not the gustatory ones, are those
stimulated by substances in dilute solution such as meat juice.
When the stimulus is prevented from reaching the former, or
when their connections with the brain are severed, the results
are the same, viz., the seizing or snapping reaction does not
take place, provided, of course, it is not induced by a stimulus
of quite a different character. I conclude, therefore, that Die-
myctylus reacts to olfactory stimuli, and that the sense of
smell plays an important part in food recognition.

The fact that a newt often seizes an inedible object such as
filter paper only when it is in motion demands some explana-
tion. In many cases this reaction probably occurs because the
motion of the object interferes with a satisfactory nosing of it,
and accordingly it is taken into the mouth where it may be
tested by taste. An animal following and attempting to scent
a moving piece of cotton or filter paper was of frequent occur-
rence, and very often the seizing reaction followed. In other
instances, however, there appeared to be no attempt on the
part of the animal to test the edibility of the object by the
sense of smell, for it was immediately pursued and secured.
An explanation of this behavior, I believe, is to be found in
the character of the natural food of the animal. An examina-
tion was made of the stomach contents of a dozen newts which

were collected in the early part of August. Admitting of identi-
fication were four snails of two genera, one water-boatman
(Corisa), one caddis-worm, several midge larvae (Chironomidae)
and three amphipod crustaceans. Since Diemyctylus feeds
upon such actively moving organisms as crustaceans and cer-
tain insects it must in many instances have no time to use its
olfactory organs, but would depend wholly on its powers of
vision and quickness of action in securing its prey; and in
consequence of such experience in nature it might be expected
to snap at a bit of moving filter paper in the aquarium. On
the other hand, the presence of insect larvae concealed in their
cases, or snails within their shells, may possibly have been
recognized by the sense of smell.

REESE'S EXPERIMENTS

Some of the work of Reese on the reactions of Diemyctylus
to chemical stimuli, together with certain conclusion, may be
briefly reviewed. Reese records the results of experiments
planned to show whether sight or the telaesthetic sense is the
one used by the newt in finding food. The animals were first
tested by comparing their reactions to pieces of meat with those
to inedible objects such as cloth, cotton and filter paper held
in forceps. Secondly, the effect of meat juice squirted over the
snout was studied.

In testing the animals by the first method the motion factor
enters, a thing which evidently the author did not attempt to
control. On page 191 he writes: "It would follow small taste-
less objects of various colors and would often seize them, but
usually disgorged them immediately," and after experimenting
with meat juice squirted over the nostrils concludes: "In secur-
ing food, then, it is probable that Diemyctylus uses both sight
and the telaesthetic sense, perhaps the latter more than the
former." Unquestionably both sight and smell are used in
obtaining food, and I believe I have shown the part played by
each. The seizing reaction undoubtedly was called forth in
many instances not so much by sight of the object as by sight
of it *in motion*. If the animals had been tested with stationary
objects by methods such as I have described, their marked
ability to discriminate perfectly between the edible and inedible
prior to seizure would in all probability have been demonstrated.

The reactions I obtained by squirting meat juice over the nostrils were like those reported by Reese.

After describing the reactions of Diemyctylus to "liquid chemicals," Reese attempts to distinguish between the sense of smell and taste. The time during which the feeding response is inhibited after cocaine is applied to the external nares is compared with similar inhibition occurring after temporarily placing cotton soaked in cocaine within the animal's mouth. The results obtained are not convincing, for, as the author points out, they are "somewhat conflicting" and open to several interpretations.

As a final test, the olfactory nerves were cut. The method is described as follows: "It was found by dissecting preserved specimens that, by inserting the points of a fine pair of scissors into the two posterior nares, and cutting the intervening tissues, both olfactory nerves could be sectioned with one quick cut. With the four selected animals this was done, after administering just enough ether to keep them from struggling." Reese's experiments upon the three animals which survived the operation and his conclusions are as follows. "These three recovered from the ether in a few minutes and the morning after the operation they were as active as ever, and gave no indication of being any the worse for the operation. Once or twice a day for more than a week they were tested with a bit of raw meat, but in no case attempted to seize it. Two of the animals paid no attention whatever to the meat, while the third, on two or three occasions, followed the meat (and also a piece of filter paper) without snapping at it. Juice from raw meat and from earthworms, described above caused no reaction whatever, though samples of both caused the snapping response in normal animals.

"After having been without food for about two weeks the animals, stimulated by extreme hunger, began to snap at meat or filter paper that was moved near them. If permitted to do so they would swallow the filter paper as readily as the meat. They would not seize either meat or paper unless it was in motion.

"While it is hard to understand why, if sight be the sense used, cutting the olfactory nerves should make an animal less apt to *follow* a bit of meat or a tasteless piece of paper, the

absolute refusal of these animals to eat, after severance of the olfactory nerves, seems to show that the olfactory sense is the one mainly used by Diemyctylus in recognizing food."

The behavior of these animals does not justify, to my mind, the author's conclusion. The nearly complete disappearance of the following reaction, and "the absolute refusal of these animals to eat" seems to show little else than a general physiological disturbance following the operations. Of the three newts, whose olfactory nerves I severed, one followed and snapped at meat in a few minutes after the operation, and the other two did the same on the day succeeding. Such reactions only showed that the animals after the operations were behaving in respect to a *moving* object as they had before, thus giving evidence that they were, in general, physiologically normal, and accordingly in suitable condition for experimental study by the methods already described. Judging from the behavior of my animals, it seems certain, therefore, that those operated upon by Reese really did give evidence of being "the worse for the operation," and accordingly their failure to respond to juice from meat and earthworms might have been attributed to causes other than non-functioning olfactory organs. My method of operating was quite unlike that of Reese, a fact explaining, in all probability, the differences in behavior described.

SUMMARY

1. An inedible object, such as a ball of filter paper, suspended in an aquarium is discovered, approached and nosed by Diemyctylus as quickly as a piece of meat.

2. A cheese cloth bag containing meat is approached, nosed and snapped at, whereas a similar one filled with cheese cloth is approached and nosed but no attempt is made to seize it.

3. When an extract of raw beef is squirted over the external nares, the newt responds by snapping its jaws or biting at the sand on the bottom of the aquarium where the juice settles.

4. When the peripheral olfactory apparatus is rendered inoperative, both bags are approached and nosed but neither is seized, and beef extract squirted over the snout excites no reaction.

5. A moving inedible object may be seized or even swallowed, whereas the same thing stationary is nosed and rejected.

CONCLUSIONS

1. The approach to an object, such as cotton, filter paper or meat, is a visual response.

2. When the object is nosed its edibility is being tested by the sense of smell.

3. Snapping at beef juice and stationary edible objects are reactions dependent on stimulation of the olfactory receptors by substances in dilute solution.

4. The seizure of a moving inedible object is a reaction probably correlated with the character of the natural food of the newt. In all likelihood sight is the sense used by Diemyctylus in the capture of actively moving organisms, whereas other food located through vision is often recognized as such by the sense of smell.

REFERENCES

BAGLIONI, S.   Contributions expérimentales à la physiologie du sens olfactif et
    1909.   du sens tactile des animaux marins.   Archiv. Ital. de Biol., T. 52,
                 pp. 225–230.
COPELAND, M.   The olfactory reactions of the puffer or swellfish, Spheroides macu-
    1912.   latus (Bloch and Schneider).   Jour. Exp. Zoöl., vol. 12, pp. 363–368.
PARKER, G. H.   Olfactory reactions in fishes.   Jour. Exp. Zoöl., vol. 8, pp. 535–
    1910.   542.
    1911.   The olfactory reactions of the common killifish, Fundulus hetero-
                 clitus (Linn.).   Jour. Exp. Zoöl., vol. 10, pp. 1–5.
REESE, A. M.   Food and chemical reactions of the spotted newt, Diemyctylus
    1912.   viridescens.   Jour. An. Behavior, vol. 2, pp. 190–208.
SHELDON, R. E.   The sense of smell in Selachians.   Jour. Exp. Zoöl., vol. 10,
    1911.   pp. 51–62.

MATURATION AND USE IN THE DEVELOPMENT
OF AN INSTINCT

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Two figures

INTRODUCTION

The work reported in this paper was done in the Psychological Laboratory of the University of Michigan during June, July and August, 1912. The problem grew in a very natural way out of a previous study[1] of the pecking instinct in barred Plymouth Rock chicks. In this earlier work a method was devised whereby the course of development in accuracy of the pecking reaction was satisfactorily traced. After the developmental curve of the instinct had been found, the question arose as to how much of the improvement from day to day is attributable to practice and how much is due to maturation apart from practice. To quote from an earlier article:

"One sometimes speaks of the *modifiability* of an instinctive action like that of pecking, but wherever this term has been employed in this paper in connection with instinct no more has been mplied than the objective fact of improvement in accuracy. an increasingly successful adjustment of parts in a more comprehensive function. The problem still remains, Is this development dependent upon practice, or is it the natural functional correlate of structural maturation independent of practice? Swallows are reported to be able to fly without previous practice. If the pecking of chicks could be successfully inhibited for a week's time without doing violence to the normal physical condition of the animals, would the accuracy of the reactions at the end of that time average 36.67 on a scale of 50, the average for our lot of twenty-one? There is evidence in support of the belief that such chicks would very quickly be pecking with average efficiency, without anything like the

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[1] F. S. Breed. The development of certain instincts and habits in chicks. *Behavior Monographs*, 1911, vol. 1, no. 1, p. 14 ff.

amount of practice chicks would have had by this time when growing naturally. In other words, .mprovement does not depend entirely upon practice. How much of the improvement does depend upon practice, * * * So far as the facts are concerned, the most one can say is that the development of the pecking instinct proceeds somewhat without practice and is hastened by it. Maturation and use run along in time together. No means has yet been devised of measuring the amount either factor apart from the other contributes to the development of the pecking reaction." [2]

PROBLEM AND METHOD

To devise and apply some method of separating these two factors became the problem in the experimentation we are reporting. The same variety of chicks was used as in the earlier experiments. The same make and style of incubator and brooder were also employed. As before, Cypher's Chick Food was used in the tests. The method of recording reactions and measuring accuracy was exactly the same as in the previous work. The terms striking, seizing, and swallowing denote here as there three distinguishable aspects of the chicks' feeding response, the term missing denoting failure to hit the object. As they appear in our records, the above terms have the following definite meanings: (1) Missing denotes all cases of the pecking reaction in which the bill failed to hit the particular object supplied by the experimenter; (2) striking, those cases in which the bill hit the object without seizing it; (3) seizing, cases in which the object was grasped momentarily in the bill and then dropped (not rejected); and (4) swallowing denotes what may be termed the perfect or complete reaction, the object being struck, seized, and swallowed in an errorless series or chain of movements. To facilitate the taking of records, the Arabic numerals 1, 2, 3, and 4 were employed to represent missed, struck, seized, and swallowed, respectively. Note was taken, of course, of the reactions independently of the number of food particles pecked at, for a single grain sometimes called forth a half dozen or more reactions in succession.

So much then for this study as it is related to certain previous work on the same instinct. Now a word in regard to

[2] Loc. cit., p. 40.

the method devised to test the effect of maturation apart from practice. The feeding reaction in its first intent being a response to an optical stimulus, it was thought feasible to prevent practice by keeping the animals in darkness prior to their first tests. To protect the chicks from light from the first, the incubator door was opaquely sealed before the date of hatching, and when the hatch was considered finished the animals were removed from the incubator to a dark-box during the night while the single window in the incubator room was shaded as a further precaution. The dark-box was lined with dull black cotton cloth and covered with the same material. In this box the chicks were transferred to a dark-room and kept therein, under a dark-curtained indoor brooder which had been made ready in advance. This brooder was heated by a system of galvanized sheet iron pipes which led from an oil lamp outside the dark-room to the brooder radiator and thence returned to the outside. No light from the heating apparatus was evident in the dark-room. The room in which the chicks were thus kept was entered through an adjoining dark-room so that light might not be admitted with the going and coming of the experimenters. The dark-room conditions having been satisfactorily arranged, a greater difficulty was confronted—that of artificially feeding the animals during the period of dark-room confinement. It was soon found futile and seemingly unnecessary to attempt feeding during the first twenty-four hours of this period. The chicks, as a rule, did not react positively to food on the first day. On the second day of the period, however, they usually began to take active part in the operation and made the attempt at feeding a much more successful one. The food in this case consisted of the regulation chick food, corn meal, and bread crumbs, all slightly moist. The chicks were taken from the dark-box one at a time, the body of an animal was clasped over the back in the hand of the experimenter, the bill was held open between the thumb and forefinger of this hand, and the food inserted in the mouth by the hand that was free. The chicks, when not taken too early, readily swallowed food thus administered. The amount supplied was regulated by the felt protrusion of the crops. Water was administered by a pipette gently introduced into their mouths. In this manner the chicks were fed and watered in the dark-room twice daily. The time

required for feeding was about 15 minutes per individual. It is worthy of remark here that, inasmuch as the death rate of the chicks was later found to increase with the length of the period of confinement, there seemed to be either a defect in our technique or a natural limit to this method of preventing practice.

FIRST REACTIONS IN THE LIGHT

When brought to the experiment table after their stay in the dark-room, the chicks generally stood quiet and inert for five minutes or more. The time recorded for one individual was ten minutes; for another, fourteen. To provide against possible difficulties of light adaptation under which the animals might be laboring, the members of group III were allowed to remain in the light from fifteen to thirty minutes before the first tests were undertaken. During this interval they were held by an assistant in order to prevent practice. The above control proved fortunate in view of a later observation made on the chicks of group V during their first few hours in the lighted experiment room. When placed in a dark-lined box, open at the top, half of which was shaded by the side of the box nearest an adjoining window, these chicks crowded into the shaded region. When moved repeatedly into the area of greater light intensity, they as often returned into the shade. Direct sunlight was excluded. Their phototropism had been reversed, at least temporarily, by the previous confinement in darkness, for chicks are known to be by nature positively phototropic. Coupling with this the further fact that the animals blinked noticeably on being first brought to the light, one may be inclined to conclude that the eyes of the animals were abnormally or pathologically affected during the first tests. However, even when special measures were not taken to adapt the chicks, the period of inactivity at the start gave considerable if not sufficient opportunity for adaptation. Furthermore, it is improbable that the normal accuracy of vision in these chicks was impaired, for their first records, compared with those of the standard group, show even a smaller average number of "missing" reactions.

During this period of inactivity they devoured with apparent relish food that was inserted in their mouths, and even excitedly gave the food twitter when thus fed. But left to themselves

they soon lapsed into inactivity aga'n. A turning point in
their behavior came when they succeeded in swallowing a grain
after one of their more or less indifferent pecks. Energetic
pecking thereupon ensued. The contrast between an animal's
attitudes before and after swallowing a grain of its own peck-
ing was very marked. Anthropomorphically speaking, one
might well say the trouble with the animal was that it did not
know what the food particles were or were for.
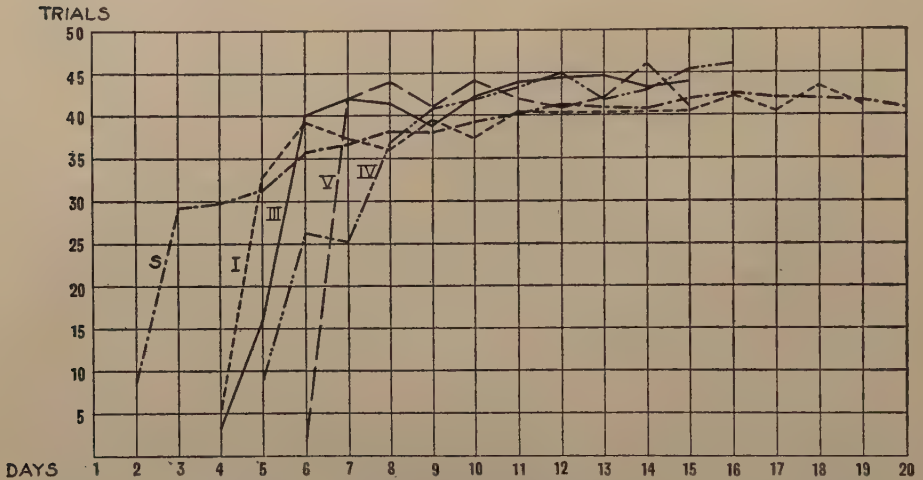
FIGURE 1.—Curves showing the course of development of the pecking instinct
    after artificial delay. S, standard curve, representing rate of improvement
    in accuracy under natural conditions. I, III, IV, and V, curves for corres-
    ponding groups of chicks in which the action of the instinct has been artifi-
    cially prevented for three, three, four, and five days respectively, previous to
    the first tests. Data in table 1.

RESULTS OF PECKING TESTS

Two lots of chicks, divided into five groups, in all twenty-
three in number, were tested. These groups were designated
by the Roman numerals I, II, III, IV, and V. In figure 1 the
corresponding curves for the several groups are designated by
these same numerals. Each curve represents the course of
development of the complete feeding coördination, that is, of
reaction 4, for a given group. The number of perfect reactions
in a series of fifty pecking reactions, the first fifty, on any given
day, is assumed to be an index of the accuracy of the pecking

instinct on that day. For purposes of comparison we have reproduced as a standard a curve of development of reaction 4, plotted from the average daily records of twenty-one chicks (curve S, figure 1). Curve I is based on similar records of four animals whose practice in pecking was prevented, as above described, up to the beginning of their fourth day. The records of group II are not presented on account mainly of their incompleteness. Curve III shows the development of the instinct in a group of six chicks, also beginning their practice on the fourth day. In curve IV are represented averages for three chicks which were released from darkness at the beginning of the fifth day. There were six individuals in this group when the first records were taken. Of six chicks originally in group V, five died within three days after the first pecking records were taken. Curve V represents the records of the one animal that completed the tests. Practice was begun in this case on the sixth day. For data see table 1.

One of the most interesting features of the results is the very low initial records of all the chicks in which the action of the instinct was delayed. Who would have predicted that the greater maturation of groups I to V would not have enabled them to begin at a higher grade of efficiency than that of the standard group on the second day? As a matter of fact, all the chicks in the above groups began their pecking as the merest novices. Once we see the value for improvement of the first attempts, the fact that these records are in every case lower than the beginning records of the standard group may well be explained on the ground that the chicks of the standard group had twenty-four hours of freedom in the light prior to their first tests, whereas the other chicks were tested immediately upon being released from the dark-room.

The next point that seems to be of rather exceptional interest is the rapidity and amount of improvement during the first two days of practice. Within this time the dark-room chicks attained the level of accuracy normal to their age. Thenceforward the progress apparently assumed its natural course. It seems probable that the peculiar change in the shape of the standard curve at the beginning of the third day is not entirely a matter of chance. The rest of the curves are of the same general character,—a rapid advance with the first two days of

TABLE 1

PECKING EFFICIENCY AFTER ARTIFICIAL DELAY, SHOWN BY THE NUMBER OF PERFECT REACTIONS IN A DAILY SERIES OF FIFTY TRIALS

Day	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	No. of chicks
Standard group		8.7	29.3	29.8	31.4	35.9	36.7	38.2	38.	39.2	40.1	41.2	40.7	40.6	41.9	42.6	42.1	41.9	41.6	40.8	21
Group I				5.5	32.8*	39.3	37.3	36.	39.5	37.3	40.5	40.5	40.3	40.5	40.5	42.3	42.3	41.5	41.3		4
Group III				3.1	16.	40.	42.	41.3	38.8	42.3	44.	44.5	44.7	43.3	44.	42.3	40.5	43.5	41.3		6
Group IV					8.7	26.3	25.3	36.7	40.7	42.	43.3	45.	41.7	43.	45.3	46.					3
Group V						2.	42.	44.	41.	44.	42.	41.	42.	46.	41.	.					1

* An evening record; morning record unsuccessful.

practice, followed by a gradual and fairly steady improvement thereafter. It seems to the writers that a given amount of practice, quite constant for the different groups, is necessary to smooth the way for the operation of a native capacity whose efficiency is largely a function of the age of the animal. In other words, it is our view that the second portion of the curves is almost entirely a record of maturation. If this be true, and further experimentation can no doubt establish the truth or falsity of this conjecture, there would seem now to be some explanation for the negative results obtained in previous tests
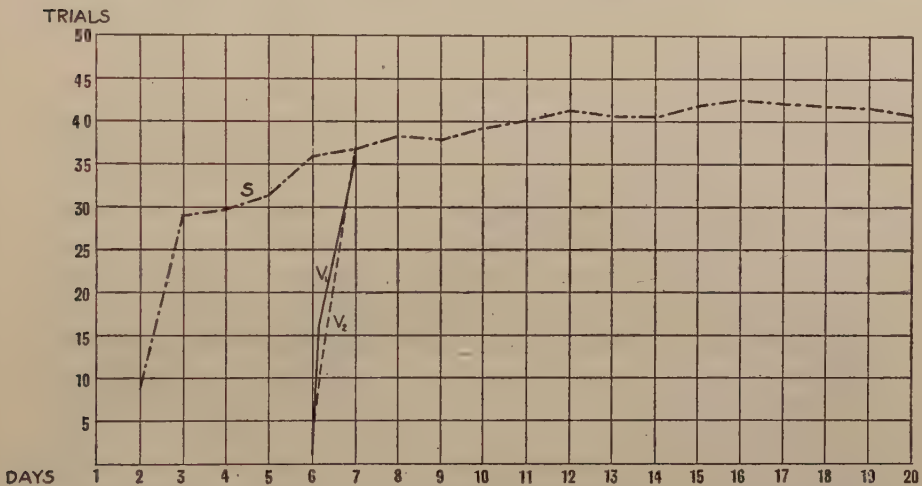
FIGURE 2.—Curves showing rate of improvement in accuracy of the pecking instinct during the first twenty-four hours after five days' artificial delay. V_1, curve for nos. 22 and 24; V_2, curve for nos. 23 and 27. S, standard curve reproduced for comparison. Data in table 2.

of the effect of social influence on the development of this instinct.

The effect of the first day's practice after an artificial delay of five days from the time of hatching is indicated in curves V_1 and V_2, figure 2. The records first given show the degree of accuracy in the first 50 trials—characteristically low. It will be noticed that as practice proceeded the improvement was both rapid and regular. Twenty-four hours from the time practice was started the individuals of these two sub-groups reached an

average accuracy score of 36 and 37 respectively on the regular scale of 50. The data are given in table 2.

TABLE 2

Rate of Improvement During the First Twenty-four Hours After Five Days' Artificial Delay, Groups V_1 and V_2

Day......................	6			7
Nos. 22 and 24, Group V_1	Time of test...... 9:58 A.M.	10:31 A. M.	1:00 P. M.	10:15 A. M.
	Perfect reactions in 50 trials........ 1.	8.5	16.	36.
Nos. 23 and 27, Group V_2	Time of test...... 10:45 A. M.	11:21 A. M.	1:00 P. M.	10:15 A. M.
	Perfect reactions in 50 trials........ 3.5	6.5	11.	37.

A critic may interpose at this point that the chicks, upon being given their natural freedom after the first tests, improved as they did because they made up lost practice in excessive pecking. Careful observation of the animals did not attest this view. Furthermore, there are two valid objections to this objection: (1) Practice was necessarily limited by the food allowance, and (2) practice was also limited by the food capacity of the animals.

An answer to one more possible objection. One of the writers found in previous experiments that chicks peck at and eat food when light is excluded. On the basis of this fact it may be supposed that dark-room conditions do not prevent the pecking response and therefore practice. It should be noted, however, that, in the earlier experiments referred to, all the animals had practiced previously in the light. No evidence of pecking was found while groups I to V were in the dark-room. These chicks did not even peck for food while being artificially fed.

CRITICISM OF SPALDING

The accuracy of the first pecking reactions after artificial delay has surely been much exaggerated by Spalding. It will be recalled that Spalding employed a hooding device to prevent practice for from one to three days. The hooding was intended to permit the chicks to acquire enough control over their mus-

cles to enable them to give evidence of their instinctive power.
If Spalding's device securely protected the eyes of his chicks
from the light, which was apparently not true in all cases, we
are inclined to believe that the first attempts of those animals
would have been found upon careful study little if any more
accurate than the efforts of his day-old chicks.

We are forced to conclude that another observation of Spald-
ing has been too freely generalized.   In his original article
appears the following:

"Something more curious, and of a different kind, came to
light in the case of three chickens that I had kept hooded until
nearly four days old—a longer time than any I have yet spoken
of.   Each of these on being unhooded evinced the greatest
terror of me, dashing off in the opposite direction whenever
I sought to approach it.   The table on which they were un-
hooded stood before a window, and each in its turn beat against
the glass like a wild bird.   One of them darted behind some
books, and squeezing itself into a corner, remained cowering for
a length of time.   We might guess at the meaning of this strange
and exceptional wildness; but the odd fact is enough for my
present purpose.   Whatever might have been the meaning of
this marked change in their mental constitution—had they been
unhooded on the previous day they would have run to me
instead of from me—it could not have been the effect of experi-
ence; it must have resulted wholly from changes in their own
organization." [3]

James, after quoting the above passage, does not hesitate,
as Spalding does, to supply the meaning of "this strange and
exceptional wildness:"

"Their case was precisely analogous to that of the Adirondack
calves (of which James had been told by farmers in the Adi-
rondack wilderness).   The two opposite instincts relative to
the same object ripen in succession.   If the first one engenders
a habit, that habit will inhibit the application of the second
instinct to that object.   All animals are tame during the earliest
phase of their infancy.   Habits formed then limit the effects of
whatever instincts of wildness may later be evolved." [4]

[3] D. A. Spalding.   Instinct.   With original observations on young animals.
Macmillan's Magazine, 1873, p. 289.
[4] William James.   Principles of Psychology, vol. II, p. 397.

In the course of our observations chicks were taken from the dark-room and brought to the experiment table at the beginning of the fourth, fifth, and sixth days, as previously detailed. The pecking tests were conducted directly before a window, the animals working at a distance of about two feet from the glass. True these chicks had not been hooded. But in no case was an animal observed to run at the approach of the experimenter or show more excessive signs of fear. Generalization on the basis of these facts reported by Spalding seems highly premature.

SUMMARY AND CONCLUSIONS

In a previous study of the pecking instinct of barred Plymouth Rock chicks data were gathered from which a curve of development of the instinct was plotted. This curve represents the improvement in accuracy of the pecking coördination from the second to the twenty-fifth day. With this curve as a standard an attempt was made to determine the relative amounts contributed by maturation and use to this improvement. Two lots of chicks, divided into five groups, in all twenty-three in number, were tested. By confinement in a dark-room prior to the first tests practice was prevented for a definite time in each group—three, four, or five days from date of hatching. Meanwhile the animals were fed and watered artificially. The most interesting features of the results are (1) the uniformly poor initial records, and (2) the rapidity with which normal accuracy was attained. Regardless of the duration of the period of confinement, within the limits specified, the chicks began below an efficiency of 18% and with from one to two days' practice reached normal efficiency. In the first two days of the curves the necessary practice component, it seems, is represented, and in the remainder a record mainly of maturation.

The improvement ensuing upon the first practice was both rapid and regular. The rapidity was not due to excessive practice following the enforced delay of the instinct. If, as suggested, the curve after the first two days is mainly a record of maturation, there would seem to be some explanation for the negative results in previous tests of the effect of social influence on the development of this instinct.

Although chicks with previous practice have been found to

eat in the dark, these chicks were not observed to peck during the period of dark-room confinement.

Shortly after being brought to the light, chicks that had been in the dark-room five days were found to be negatively phototropic, at least temporarily.

Spalding has exaggerated the accuracy of pecking after artificial delay. The manifestations of fear after four days in darkness, as reported by Spalding, were not observed in these chicks.

THE HEREDITY OF SAVAGENESS AND WILDNESS IN RATS

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In 1910 Professor William E. Castle suggested to me the desirability of studying the heritability of savageness and wildness in certain strains of rats which were being bred for studies in the heredity of structural characteristics at the Bussey Institution. I undertook the proposed investigation, and by the generous aid of Professor Castle and Doctor John C. Phillips I have been enabled to test the behavior of nearly three hundred individuals. The investigation is incomplete, and in this paper I propose to present merely a preliminary report, reserving the detailed account of my work, with the experimental data, for a paper to be published later, in some journal of genetics.

At the outset I made a preliminary analysis of the behavior of some of the rats in order to discover several traits which seemed to be fairly isolable and capable of reasonably accurate measurement. As a result of these observations, I decided to make tests of the savageness, wildness, and timidity of wild rats, tame rats, and of the first and second generation hybrids.

Preliminary attempts at measurement indicated that six grades, with respect to these several traits, might be utilized. These grades I designated as o, 1, 2, 3, 4, and 5. The grade o indicates the absence of the various signs of savageness, wildness, or timidity. The grade 5 indicates the presence of these signs in maximal number and intensity.

In order to obtain a rough measure of the reliability of my judgments, I tested the individuals of several litters of rats with respect to the three traits designated and, later, without knowledge of my previous results, retested the same individuals. A comparison of the measurements thus obtained indicated that they were often the same and seldom differed by more than a grade. The results given below are typical.

No. of rat	Date	Savageness	Wildness	Timidity
59♂	Jan. 10......................	4	4	3
	" 17......................	4	3	2
69♀	" 10......................	3	3	4
	" 17......................	2	3	4
63♀	" 10......................	3	3	4
	" 17......................	3	3	3

The preliminary analysis of the behavior of rats, and my measurements, convinced me that I might profitably undertake a systematic study of savageness, wildness, and timidity in wild, tame, and hybrid individuals. Of these three traits, or possibly I should say, combinations of traits, timidity is the most difficult to recognize and satisfactorily measure. It is indeed extremely doubtful whether it can with sufficient certainty be distinguished from wildness to render measurements significant. I have attempted, however, throughout the investigation, to measure it and I shall report the results along with those for the other traits.

My method of testing the rats was to place a cage containing individuals to be examined on a table in the center of an otherwise unoccupied room. I then removed an individual from the cage in order carefully to observe its behavior. This removal was effected by means of my gloved hand, when that method could safely be used, or, in the case of extremely savage animals, by means of a pair of placental forceps which were used to grasp the animal by the tail.

The chief indications of savageness noted and relied upon as a basis for grading are (1) biting; (2) exposing or gnashing the teeth; (3) jumping at hand or forceps; (4) squeaking.

Similarly, the chief indications of wildness are (1) attempts to hide from view in cage or in hand; (2) random and excited running about in the cage or excited attempts to escape from the hand or the forceps; (3) squeaking; (4) urination and defecation.

Timidity is indicated (1) by attempts to avoid the experimenter; (2) by a kind of chattering or gnashing of the teeth; (3) by cowering and what looks like trembling; (4) urination and defecation.

From my notes, I reproduce the following statements concerning these several traits of behavior. "Savageness is of two

kinds, defensive and offensive. Of each there are several indications. The former deserves a higher grade than the latter. Defensively savage individuals are likely to jump at the observer and cannot be safely handled even with the gloved hand. Offensively savage rats may safely be handled: it is necessary only to avoid hurting them. Wildness almost invariably accompanies savageness. Timidity may or may not. An extremely savage and wild rat may exhibit little fear of the experimenter. A savage and aggressive wild rat fights, whereas a timid rat cowers, trembles, and chatters."

The animals observed, numbering about three hundred (300), consisted of wild rats, tame rats, and first and second generation hybrids.

The wild rats were captured either in Belmont or in Cambridge, Massachusetts, and were, with one exception, adult males. Observations and tests on them, made in several instances immediately after capture and again after they had been in captivity for a year, indicated extreme savageness and wildness, with variable timidity. The grade of 5 for savageness was assigned to almost all of these individuals. In wildness, they were graded either 4 or 5, and in timidity 3, 4, or 5. As a result of their confinement in cages for a year, they exhibited a lower grade of wildness and timidity, but their savageness remained unchanged. It was impracticable and wholly unnecessary to repeat frequently the tests on these wild individuals.

The tame rats were taken from a strain in use for studies of coat color at the Bussey Institution. This strain has been bred in the Harvard Zoölogical Laboratory for at least ten years. A brief account of some recent experiments with these rats has been given by Professor Castle in a paper entitled "Some biological principles of animal breeding." [1]

As a result of certain experiments in selective breeding, two types of animal, each of which was used in my experiments are distinguishable in this strain. They are known as wide (W) and narrow (N) individuals. Both have black heads (hoods), but in the wide the black extends further back than in the narrow. The wide are known to have more wild blood than the narrow, and in these experiments they prove to be wilder and more savage.

[1] *American Breeders' Magazine*, 1912, vol. 3, no. 4.

Tests of savageness, wildness, and timidity were made with eight male and eighteen female tame rats (some wide, some narrow). Each individual was tested twice, the tests being separated by an interval of one month. In no case did the males receive a grade above o. All were so gentle and tame that they could readily be taken up in the ungloved hand and examined. The females were decidedly less gentle and tame than the males. Two of the eighteen tested received a grade of 1 for savageness and fourteen of the eighteen received a grade of 1 for wildness. The number receiving a grade above o for timidity was twelve.

The contrast between the wild and the tame rats with respect to savageness, wildness, and timidity is extremely marked.

The first generation of hybrids was obtained in almost all cases by crossing a wild male with a tame female. This mating is much more satisfactory, because more likely to yield offspring, than the mating of a wild female with a tame male. By crossing the first generation hybrids among themselves, without selection with respect to savageness, wildness, and timidity, the second generation hybrids were obtained. Up to the present, no third generation hybrids have been examined.

As the mating, numbering, and weaning of the rats used were attended to by Professor Castle and Doctor Phillips, the experimenter was wholly unprejudiced, while making his tests, by knowledge of the genetic relations of the individuals. Very rarely indeed did he know whether the individual under observation was a tame rat or a first or second generation hybrid rat. Thus, he was able to escape entirely the influence of possible presuppositions concerning the behavior of savageness, wildness, and timidity in heredity.

With a few exceptions, each individual was tested from three to five times, at intervals of several days. The first test was made, as a rule, at the age of about six weeks and the remaining tests usually covered a period of at least a month, sometimes two months. It was noted that in general the animals receive lower grades with repetitions of the tests. This is due in part to the experience of being handled, but even more to the fact that they become accustomed to seeing human beings and to being disturbed when fed or when the cages are cleaned. There is also some evidence that ageing has something to do with the change.

TABLE 1

RESULTS OF SUCCESSIVE TESTS OF FIRST GENERATION, F₁, (NARROW × WILD) HYBRID RATS

Rat	Age	Date	Savageness	Wildness	Timidity
19♀	58 days	Sept. 25	5	4	4
		Oct. 2	4	4	3
		" 17	3	4	3
		" 24	1	2	2
20♀	58 days	Sept. 25	5	5	5
		Oct. 2	5	5	4
		" 17	5	5	4
		" 24	2	3	2
94♀	45 days	Aug. 7	1	1	1
		" 13	2	2	2
		Sept. 25	1	3	2
		Oct. 2	0	2	2
21♂	58 days	Sept. 25	5	5	5
		Oct. 2	4	4	3
		" 17	3	4	3
		" 24	2	3	2
22♂	58 days	Sept. 25	5	4	4
		Oct. 2	4	4	3
		" 17	2	3	2
		" 24	.1	3	2
80♂	50 days	June 20	0	2	1
		July 5	1	2	1
		Aug. 7	0	1	1
		" 13	0	0	0

In tables 1 and 2 are presented, for contrast, typical results obtained with groups of F₁ (first hybrid generation) males and females and F₂ (second hybrid generation) males and females. The individuals of these tables are all the offspring of crosses between narrow tame rats and wild rats. The results indicate (1) diminishing savageness, wildness, and timidity with repetitions of the tests; (2) sex differences; (3) marked differences for the two generations. The F₁ individuals grade much higher, on the average, in savageness, wildness, and timidity than do the F₂ rats.

The results for seventy-eight F₁ individuals are summarized in table 3. All of these individuals were the offspring of narrow by wild crosses. The table presents, in the first horizontal line, (a) the average age; (b) the range in age of the group; (c) the average number of tests; (d) the range of tests; (e) the average

TABLE 2

RESULTS OF SUCCESSIVE TESTS OF SECOND GENERATION, F_2, (NARROW × WILD)
HYBRID RATS

Rat	Age	Date		Savageness	Wildness	Timidity
52♀	75 days	Jan. 24		0	3	2
		" 31		0	1	1
		Mar. 11		0	0	0
		" 18		0	0	0
55♀	75 days	Jan. 24		0	0	1
		" 31		0	0	0
		Mar. 11		0	0	0
		" 18		0	0	0
64♀	80 days	Sept. 27		5	5	5
		Oct. 7		5	4	4
56♂	75 days	Jan. 24		0	3	2
		" 31		0	2	2
		Mar. 11		0	1	1
		" 18		0	1	1
53♂	75 days	Jan. 24		0	3	2
		" 31		0	3	3
		Mar. 11		0	3	1
		" 18		0	2	1
62♂	80 days	Sept. 27		5	5	5
		Oct. 7		5	5	4

grade attained in the first test for savageness; (f) in the last
test for savageness; (g) the average grade for all tests (that
is the average for the total number of tests given to the group).
The same three values are given also for wildness and for tim-
idity. Immediately below these averages appears the distribu-
tion of the rats in the grades 0 to 5.

In tables 3, 4, 5, and 6 the results for males and females are
presented separately. Tables 3 and 4 present the results ob-
tained from the offspring of narrow tame by wild crosses, and
tables 5 and 6 those obtained from the offspring of wide tame
by wild crosses.

It is apparent from table 3 that the F_1 narrow by wild indi-
viduals of both sexes grade high in savageness, wildness, and
timidity. Without exception, the females grade higher than
the males. Thus, the first test for savageness yielded the grade
of 4.39 for the females and 3.45 for the males. This result is
typical. The lower grades attained in the last test are note-
worthy. If we designate the grade which is most frequent as

TABLE 3

SUMMARY OF RESULTS FOR FIRST GENERATION HYBRIDS, F₁ (NARROW × WILD)

		Age		No. of tests		Savageness			Wildness			Timidity		
		Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Last Av.
		42.45	25–91 da.	3.76	2–4	3.45	1.6	2.52	4.19	2.86	3.24	3.74	2.43	2.9
42 F₁ males	Distribution of rats in grades 0–5.				0	4	19	5	0	1	0	0	1	0
					1	6	3	8	0	4	1	2	9	3
					2	2	3	6	3	10	10	7	10	14
					3	2	10	9	6	12	11	6	15	9
					4	11	7	11	13	15	18	12	7	16
					5	17	0	3	20	0	2	15	0	0
		48.75	25–81 da.	3.67	1–4	4.39	2.5	3.31	4.44	3.22	3.78	4.19	2.64	3.33
36 F₁ females	Distribution of rats in grades 0–5.				0	0	7	0	0	0	0	0	0	0
					1	2	6	3	1	2	0	1	6	0
					2	0	5	7	0	10	3	3	13	10
					3	3	4	8	3	9	11	2	7	8
					4	8	8	12	10	8	13	12	8	14
					5	23	6	6	22	7	9	18	2	4

the modal grade, we have, in the case of the first tests for both males and females, 5 as the modal grade. In other words, this group of F₁ hybrids attain the maximal grade of savageness, wildness, and timidity with modal frequency.

Turning now to a comparison of the results of table 3 with those for the second generation hybrids as presented in table 4, we discover, first of all, that the F₂ individuals, numbering one hundred and fifteen, grade very much lower on the average in savageness, wildness, and timidity than do the F₁ hybrids. A comparison of the results for the two sexes indicates a marked difference in that, whereas the F₁ females grade higher than the males, the F₂ males grade higher than the females. With respect to the distribution of individuals there is a great difference for the two generations, for whereas the F₁ individuals attain as their mode the grade of 5, the F₂ individuals in no instance attain a grade higher than 2 as the modal grade, and in most cases it is either 0 or 1. Thus, it may be noted in table

TABLE 4

Summary of Results for Second Generation Hybrids, F$_2$ (Narrow × Wild)

	Age		No. of tests		Savageness			Wildness			Timidity		
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.
	50.76	21–112 da.	4.	1–7	1.37	.7	1.	2.37	1.83	2.04	1.91	1.46	1.63
46 F$_2$ males	Distribution of rats in grades 0–5.	0	24	34	27	0	6	2	1	8	2
		1	7	5	7	12	17	15	17	22	24
		2	2	1	5	16	12	17	20	7	13
		3	4	2	2	11	4	6	4	6	3
		4	5	1	2	3	4	3	1	2	4
		5	4	3	3	4	3	3	3	1	0
	46.87	21–112 da.	4.2	1–7	1.17	.49	.74	2.17	1.52	1.77	1.84	1.28	1.42
69 F$_2$ females	Distribution of rats in grades 0–5.	0	37	56	45	2	10	8	1	10	7
		1	11	4	10	20	26	17	30	37	33
		2	6	1	6	21	23	31	25	17	24
		3	5	5	4	19	7	9	8	3	3
		4	7	2	3	4	3	4	2	2	2
		5	3	1	1	3	0	0	3	0	0

4, that the modal grade for all of the averages under savageness is o; under wildness or timidity, 1 or 2.

The results of tables 5 and 6 stand in striking contrast with those of tables 3 and 4. Although the numbers of individuals resulting from wide by wild crosses are small for both the first and the second generation hybrids, the differences which appear from comparison of tables 3 and 4 with 5 and 6 indicate clearly the influence of the wild blood in the wide tame parent.

We note from table 5 that fifteen F$_1$ individuals yield average grades which are about as high as those for the F$_1$ narrow by wild. There is slight difference, however, in the case of the F$_1$ wide by wild individuals for the sexes. The modal grade for savageness, wildness, and timidity is seldom below 4 for either males or females. In two cases it is 3.

The second generation of wide by wild individuals grades nearly as high as the first generation and is thus in marked contrast with the second generation of the narrow by wild

TABLE 5

Summary of Results for First Generation Hybrids, F₁ (Wide × Wild)

	Age		No. of tests		Savageness			Wildness			Timidity		
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.
t.	42	40–46 da.	3.44	1–5	3.78	2.78	3.44	4.44	3.11	3.78	4.	2.89	3.44
9 F₁ males	Distribution of rats in grades 0–5.	0	0	3	0	0	0	0	0	0	0
		1	1	0	3	0	3	0	0	0	0
		2	1	0	0	0	0	3	2	3	3
		3	1	0	0	2	0	0	1	5	0
		4	2	5	2	1	5	2	1	0	5
		5	4	1	4	6	1	4	5	1	1
	43	40–46 da.	4.00	3–5	4.	2.5	3.33	4.33	2.83	3.67	3.83	2.5	3.17
6 F₁ females	Distribution of rats in grades 0–5.	0	0	2	0	0	0	0	0	0	0
		1	1	0	2	0	2	0	0	1	0
		2	0	0	0	0	1	2	2	1	2
		3	0	1	1	2	1	1	0	4	1
		4	2	3	0	0	3	0	1	0	3
		5	3	0	3	4	0	3	3	0	0

rats. There is no very marked constant difference for the sex groups. The modal grade ranges from 0 to 4. It is most frequently either 2 or 3. This apparently indicates that savageness, wildness, and timidity are of lower grade development in the second generation than in the first generation of wide by wild individuals.

Finally, in table 7 are presented the results for the F₁ as contrasted with the F₂ narrow by wild hybrids, the sex groups having been combined. In this table, the averages for the first test alone are given. This test appears to be in all respects the most reliable measurement of the several traits. As appears, the first generation hybrids approximate the average grade of 4 in savageness, wildness, and timidity, whereas the second generation hybrids approximate the average grade of 2. The modal grade for the first generation individuals is 5 in the case of all three traits. For the second generation individuals it is 0 in the case of savageness and 2 in the cases of wildness and timidity.

TABLE 6

SUMMARY OF RESULTS FOR SECOND GENERATION HYBRIDS, F$_2$ (WIDE × WILD)

	Age		No. of tests		Savageness			Wildness			Timidity		
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.
	34.88	26–50 da.	3.18	1–4	3.06	1.82	2.24	3.71	3.29	3.35	3.18	2.71	2.65
17 F$_2$ males — Distribution of rats in grades 0–5. 0					2	6	2	0	0	0	0	0	0
1					0	3	3	0	0	0	1	1	1
2					2	2	6	1	3	2	4	7	6
3					7	3	3	5	7	8	5	5	8
4					3	0	1	9	6	6	5	4	2
5					3	3	2	2	1	1	2	0	0
	40.09	26–50 da.	2.96	1–4	3.26	2.13	2.65	3.74	3.09	3.3	3.3	2.35	2.74
23 F$_2$ females — Distribution of rats in grades 0–5. 0					1	5	2	0	0	0	0	0	0
1					3	3	2	0	1	0	1	4	1
2					2	7	7	2	5	4	5	12	11
3					4	2	5	6	12	11	6	4	6
4					9	4	5	11	1	5	8	1	3
5					4	2	2	4	4	3	3	2	2

TABLE 7

GENERAL SUMMARY OF RESULTS FOR FIRST AND SECOND GENERATION
HYBRIDS, (NARROW × WILD)

78 F$_1$ males and females	Savageness first test 3.92	Wildness first test 4.33	Timidity first test 3.97
Distribution of rats in grades 0–5. 0	4	0	0
1	8	1	3
2	2	3	10
3	5	9	8
4	19	23	24
5	40	42	33

115 F$_2$ males and females	Savageness first test 1.27	Wildness first test 2.27	Timidity first test 1.88
Distribution of rats in grades 0–5. 0	61	2	2
1	18	32	47
2	8	37	45
3	9	30	12
4	12	7	3
5	7	7	6

The results thus briefly presented in tabular form prove conclusively that savageness, wildness, and timidity are heritable behavior-complexes. It is hoped that the further study of these characteristics in the third generation hybrids, and in special matings from the first and second generation hybrids, may yield more definite results concerning the modes of transmission.

NOTES

NOTE ON THE SEX BEHAVIOR OF THE POITOU JACKS

RAYMOND PEARL

The practical live-stock breeder is in a position to make many interesting and important observations regarding the animals with which he works. What farmers and breeders have put on record furnishes practically the only basic data for the building of a comparative psychology of the larger domestic animals (*teste* the writings of Darwin, Romanes, Groos, and others). Unfortunately, however, the amount of such raw material for comparative psychological analysis and synthesis which has been made available to the trained psychologist by getting into the literary channels which are familiar or accessible to him, is only a small fraction of the total existing amount. The majority of farmers and stockmen are not prolific authors. Furthermore when their observations are published they are, in nearly every instance, printed in some agricultural paper, where they are most unlikely ever to come to the attention of the psychologist, and where they are at once practically lost for any purpose, owing to the ephemeral character of most such papers.

In view of these considerations I venture to call the attention of the readers of this journal to some observations which seem to me to be of a good deal of interest and value to the student of the comparative psychology of sex. These observations were recently reported in the Breeders' Gazette* (Chicago), by Mr. John Ashton, a European representative of the Gazette, and an accurate and well-informed writer on live-stock matters. The observations here recorded were made by Mr. Ashton during a visit to the famous mule-breeding district of France, Poitou. Here are bred mules of an especially valuable sort, and it was to study this mule breeding industry at first-hand that Mr. Ashton went there. In the course of a very interesting general

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account of the industry he describes the breeding behavior of the jacks in the following words:

"The jack farm has a large stone breeding barn having closely boarded boxes, in which the jacks are kept. These boxes have no windows and no openings, except a small space left between the ends of the boards and the ceiling for ventilating purposes, the result being that the animals are kept in total darkness. Each box is closed by a strong door, secured by a lock or bars and chains. At one end of the barn a peculiar yet simple arrangement to expedite breeding operations is constructed. Its general appearance resembles a pair of extremely heavy, long, narrow wagon shafts. * * *

"After the mare is placed, with her head to the wall, between these shafts, the manure and straw lying on the ground in her rear are heaped up, or scraped away, in conformity with the size of the mare or jack. The close hitching of the mare to the cross-piece, together with the narrow width of the shafts, does not allow her to move about. If she shows any disposition to kick, hobbles are placed around her fetlocks. The tail is carefully arranged so as to keep it out of the way. Notwithstanding the great lascivity of the jack, he only demonstrates it in the presence of his natural female, and for this reason artificial means have to be introduced to prepare him for serving mares. These means vary somewhat in the different studs, but by far the most popular is that of singing a song known as the "trelandage," owing to it being composed of the monosyllables *tre* and *la*. This was the method of preparing the jacks on the first farm that I visited. At some haras the pronunciation of certain words in a peculiar tone seems to be in favor; at others a jerky whistling sound is practiced to achieve the desired object, while in some cases the simple act of clanging the door chains, turning the lock, or opening and closing the door several times causes the jack to manifest a keen desire. There are even some, but not many I am told, that do their duty at the first call, without any of these comical formalities.

"Sometimes it is found necessary to allow the jack to see a female of his own species, the latter being replaced by a mare at the proper moment. When this is done the mare is often draped so as to deceive the jack. Only the owners or the grooms in charge of the different jacks seem to understand the indi-

vidual weaknesses and predilections of their animals. It is notorious that many of the finest jacks are the most capricious. When every other means have failed, a young jack is placed near the mare, during which time his elder but less ardent brother is firmly held at a distance. This stratagem causes the latter to exhibit the greatest fury and jealousy, and generally results in his amorous passions being aroused to the required extent. Nor does the mare, notwithstanding she has small chance of showing her objections, always, lend her willing co-operation to the misalliance with the jack. This necessitates the services of the teaser being requisitioned, after which the mare is blindfolded and a jack is substituted for the stallion. However, the jack breeders know their animals so well that very little time is lost."

These observations seem to me to bring out very clearly the following points of technical psychological interest:

1. A remarkable keenness of the sense of sex-recognition in the jack.

2. A large *psychological* element, which has certainly some points of resemblance to similar elements in human sex behavior,[1] in the sexual activity of the jack.

3. Evidence of association in the sex behavior of the jack, which would appear to be of significance in a genetic study of the origin of sex fetishism.

Biological Laboratory, Maine Agricultural Experiment Station

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[1] Cf. Havelock Ellis. The Psychology of Sex. (Philadelphia) *Passim.*

A SET OF BLIND WHITE RATS WHICH COULD NOT LEARN THE MAZE

HENRY FOSTER ADAMS

During a series of experiments performed at the University of Chicago during the years 1910 and 1911, the writer found a set of three blind rats which could not, or at least did not, learn the maze during a total of 182 trials. As no comment upon a phenomenon of like nature has come under my observation, it seemed worth while to mention it.

Originally there were seven rats, six females and one male. all mature and healthy. They were blinded by removing the eye-ball. All recovered promptly and were set to work to learn the maze.

During the course of the experiment, two of the rats died, so their records are not included. The disease which carried them off is, I believe, a relatively common one among white and black and white rats. First there is a sluggishness, a loss of appetite and general listlessness. This is followed in a day or two by other symptoms, the most prominent of which is a persistent twisting of the neck, so that the head is turned sideways in an abnormal manner. The head is turned more and more from day to day. Finally the animal finds it difficult to stand, is almost incapable of walking and, if it attempts to do so, often rolls over and over on the floor of the cage. Since the symptoms are somewhat similar to those following semicircular canal disturbances, it is thought by some that the disease is due to a "cold" which has settled in those organs. So far as I know, this is mere supposition. The disease is generally fatal, though there have been some cases in which the animal so afflicted has recovered.

One of the rats in this set which finally learned the maze was finally carried off by this trouble. During the first stages of the disease, it was still run in the maze. The animal had "forgotten" the labyrinth entirely. Not only that, but it ran at full speed ahead, bumping into blind alleys, partitions and the like. Whenever it got to its feet, it ran in the direction

in which it was headed until it brought up again against some obstruction.

Of the five rats which were left alive, two eventually learned the maze; the other three did not. Following is the time and error record, as far as it was kept, for the first 120 trials out of the total, 182. During a part of the experiments, the error record of the three rats which could do nothing with the maze was not kept as they often went up above 200 errors. The trials are averaged by 10's to save space.

Trials	Rats which did not learn maze		Rats which did learn maze	
	Time	Errors	Time	Errors
1–10.............................	14.60	88.46	6.59	38.95
11–20.............................	6.26		2.60	10.10
21–30.............................	9.90		1.39	3.70
31–40.............................	5.85		2.35	11.95
41–50.............................	4.26		1.68	6.15
51–60.............................	1.79	13.79	.81	.90
61–70.............................	2.88	25.28	1.33	3.20
71–80.............................	4.00	25.75	.95	1.70

Up to this point in the experiment, the maze had been covered with a glass top, but here a wire screen was substituted for it. The records of the rats which did not learn the maze is continued below.

	Time	Errors
81–90.....................	4.66	17.80
91–100.....................	2.66	10.50
101–110.....................	2.24	11.70
111–120.....................	2.97	18.30

The record continues until the 182nd trial. It is not given below, because the rats never came any closer to learning the maze than the above record shows.

There were certain peculiarities in the behavior of the rats throughout the experiment. They were all very slow in their movements while in the maze. The fastest time that any one rat made was 15 seconds. And that record was made by only

one rat on one trip. All of the other records were considerably slower. With another set of blind rats which were used mmediately after this set, all of the group made the trip in 10 seconds or under consistently. The time spent in getting to the food box by rat 3, which learned the maze perfectly, is quite irregular. It varies from 2.11 minutes to .30 minutes for trips without error.

Another peculiarity is the slowness with which the two seccessful rats learned the maze. The second set of blind rats, used in the same maze, were able to go five successive times without error with an average of 20 trips, the slowest one having learned it on the 23rd trial, the best on the 17th. Applying the same criterion to the first set of blind rats, we find that the better of the two had learned the maze on the 49th trial, the other on the 63rd.

To save time in the number of days spent in the learning process, I ran the rats twice a day from the 25th to the 62nd trip. This seemed to affect the two rats which did learn the maze quite differently. Rat 3 continually made more errors on the first trial than on the second, often going the second time without error. Rat 7, on the other hand, made the first trial quite successfully, generally without error, but went all to pieces on the second trip, sometimes making as many as 36 errors.

After finishing with the animals, we sent them to the Department of Neurology, for a post-mortem examination. A gross examination disclosed no defects whatsoever.

A SOCIETY FOR ANIMAL PSYCHOLOGY

ROBERT M. YERKES

In September, 1912, there was established in Elberfeld a Society for Animal Psychology, the chief purpose of which is to promote the investigation of the mental life of the mammals, and especially of dogs, apes, and elephants. It is hoped by the founders that an Institute for Experimental Animal Psychology may be established and that numerous investigations may be encouraged in various ways by the Society.

The organization consists of members, fellows, and founders. Members pay no dues; fellows pay eight marks per year; and founders make a single contribution of at least one thousand marks.

The officers of the Society are Prof. H. E. Ziegler, Stuttgart, President; Dr. P. Sarasin, Basel, Vice-President; Karl Krall, Elberfeld, Secretary; Geh. Kommerzienrat Aug. Freih. von der Heydt, Elberfeld, Treasurer; and the following directors: Dr. R. Assagioli, Florence; Prof. Dr. H. von Buttel-Reepen, Oldenburg in Gr.; Prof. Dr. H. Kraemer, Hohenheim-Stuttgart; Prof. Dr. A. Besredka, Paris; Prof. Dr. Ed. Claparède, Genf; and Dr. William Mackenzie, Genoa.

The Society has already begun the publication of proceedings, "Mitteilungen der Gesellschaft für Tierpsychologie," under the directorship of Professor Doctor H. E. Ziegler, Stuttgart. The first number of the first volume appeared in 1913. The publication is to appear quarterly and to be sent free of charge to fellows of the Society.

The first number contains, in addition to an announcement of the Society for Animal Psychology and a list of the members, two brief articles concerning the trained horses of Elberfeld. In the first of these articles, explanations by eminent authorities of the behavior of Mr. Krall's thinking horses are offered, and in the second, Mr. Krall himself presents an account of the behavior of the blind horse, Berto.

This is the first Society for the promotion of the experimental study of the psychology of animals to be founded, and it is greatly to be hoped that those who are interested in the subject, no matter where they happen to be located, may join the organization as fellows, and thus further its work and keep in touch with the progress of investigation through the proceedings of this Society.